

Assessment of SCE trends in the NH since 1978 indicates that for the October to February period there is substantial uncertainty in trends with the sign dependent on the observational product. Analysis using the NOAA Climate Data Record shows an increase in October to February SCE (Hernández-Henríquez *et al.*, 2015; Kunkel *et al.*, 2016) while analyses based on satellite borne optical sensors (Hori *et al.*, 2017) or multi-observation products (Mudryk *et al.*, 2020) show a negative trend for all seasons.

AR6 WGI Chapter 9 (p. 1284) points out that the NOAA Climate Data Record showing increased fall and winter SCE is inconsistent with land-based observations and model-based data sets. It notes that using optical satellite imagery to infer SCE in winter is challenging due to cloud cover and decreased solar illumination in winter months. Focusing on the Pacific Coast states (CA, OR and WA) cold season mountain snowfall that melts in spring and summer provides a substantial portion of warm season water resources. A comprehensive reconstruction of snowfall for the main source regions (Cascades and Sierra Nevada Mountains) indicates no significant trends in annual totals since the late 19th century (Christy 2022).

In summary, the up-to-date Rutgers SCE database indicates a mismatch between models and observations. Further work to reconcile conflicting trends in observational data sets is required.

5.7 Hemispheric symmetry of the planetary albedo

The planetary albedo is the fraction of incoming solar radiation reflected by the Earth back to space. It is an important element of the radiative energy balance and influences whether the planet will warm or cool over time. The planetary albedo is typically estimated at around 0.30; variations on the scale of 0.01 correspond to changes in solar forcing of about 3 W/m^2 , an amount larger than current anthropogenic forcing. It has long been noted that models disagree with each other and with observations on the value of the global planetary albedo (Stephens *et al.* 2015).

An intriguing property of the Earth's albedo is that, on average, the Northern Hemisphere (NH) and Southern Hemisphere (SH) have had nearly the same albedo at least throughout the fifty-year satellite record (Stephens *et al.*, 2015). This symmetry is surprising, because the SH has much more ocean than land. Since ocean is less reflective than land, the NH should have higher albedo. Clouds (which are highly reflective) are more common in the NH and so compensate the surface albedo imbalances of the two hemispheres. Datseris and Stephens (2021) show that this cloud compensation comes from the extra-tropical storm tracks of the SH, which are cloudier than those of the NH. While the mechanism for this hemispheric symmetry is unclear, it likely operates on large temporal and spatial scales.

The hemispheric symmetry of the albedo is a simple gross metric for climate models. Rugenstein and Hakuba (2023) defined that metric as the difference between the NH and SH annual mean albedos, expressed as Wm^{-2} of the reflected sunlight, and compiled it for the CMIP6 climate models, as shown in Figure 5.9. Most of the CMIP6 models do not reproduce the small observed asymmetry (about 0.1 Wm^{-2}) and even disagree as to which hemisphere is more reflective. Moreover, the magnitude of the asymmetry ranges up to 5 Wm^{-2} in some models, twice as large as the current anthropogenic forcing (about 2.5 Wm^{-2}).

The significance of unphysical albedo asymmetries in the climate models is not yet fully known. However, other model studies suggest that interhemispheric changes in albedo can alter poleward heat fluxes, meridional temperature gradients, storminess, and differences in hemispheric ocean heat storage. The discrepancy between models and observations further raises issues regarding cloud feedback processes and so more generally diminishes confidence in model projections of the future climate.

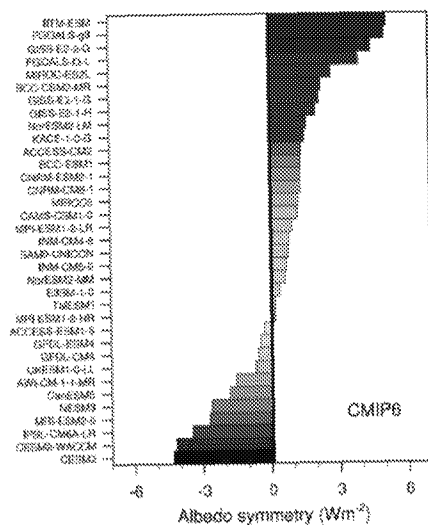


Figure 5.9. Differences in 20-year average reflectivity (albedo) between the Northern and Southern Hemispheres for CMIP6 models used in the most recent IPCC assessment (colored bars). The very small observed difference is indicated by the vertical black line. From Rugenstein and Hakuba (2023).

5.8 U.S. Corn Belt

One of the largest discrepancies between models and observations is in the U.S. Corn Belt, a region of particular importance to global food production. Figure 5.10 shows the warming trends for summertime (June, July, August) for the 12-state Corn Belt (IN, IA, IL, ND, SD, MO, MN, WI, MI, OH, KS, NE) during 1973–2022. All 36 climate models (red) warm far too rapidly compared to observations (blue).

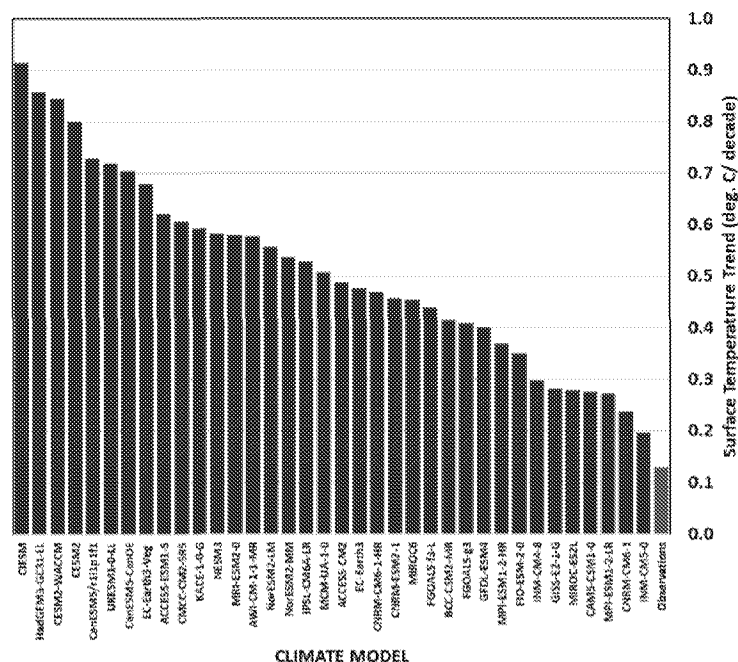


Figure 5.10: Modeled versus observed warming trends in the U.S. Corn Belt, 1973-2022.

As discussed in Chapter 9, the anticipated negative effects of increasing temperatures on U.S. corn yields have not materialized, in contrast to widely publicized studies proclaiming that theoretical future impacts are already being experienced (e.g. Seager *et al.*, 2018).

The IPCC acknowledges limitations in the accuracy of regional climate model outputs. This example shows that users need to assess model projections carefully on a case-by-case basis since local biases may be sufficiently so large that the models are simply not fit for purpose. As has recently been noted by two leaders of the modeling community (emphasis added)

... for many key applications that require regional climate model output or for assessing large-scale changes from small scale processes, we believe that **the current generation of models is not fit for purpose.** (Palmer and Stevens 2019)

To summarize:

- Climate models show warming biases in many aspects of their reproduction of the past few decades
- They produce too much warming at the surface (except in the models with lowest ECS), too much warming in the lower-and mid-troposphere and too much amplification of warming aloft
- They also produce too much stratospheric cooling, too much snow loss, and too much warming in the Corn Belt.

- The hemispheric albedo difference in individual climate models ranges widely in sign and magnitude compared to observations. The range in W/m^2 is three times larger than the direct anthropogenic forcing of CO_2 .
- The IPCC has acknowledged some of these issues but not others.

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6 EXTREME WEATHER

Chapter Summary

Most types of extreme weather exhibit no statistically significant long-term trends over the available historical record. While there has been an increase in hot days in the U.S. since the 1950s, a point emphasized by the IPCC AR6, numbers are still low relative to the 1920s and 1930s. Extreme convective storms, hurricanes, tornadoes, floods and droughts exhibit considerable natural variability but long-term increases are not detected. Some increases in extreme precipitation events can be detected in some regions over short intervals but the trends do not persist over long periods and not at the regional scale. Wildfires are not more common in the U.S. than they were in the 1980s. Burned area increased from the 1960s to the early 2000's; however it is low compared to the estimated natural baseline level. U.S. wildfire activity is strongly affected by forest management practices.

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6.1 Introduction

High impact weather extremes, usually related to temperature, precipitation and/or high wind, can disrupt infrastructure and therefore endanger human health and wellbeing. The issue is not whether extremes occur. Rather, it is whether there are long-term (decadal scale) changes in the frequency or character of extremes ("detection"), as well as the extent to which such changes and the attendant changes in hazards are caused by anthropogenic emissions of greenhouse gases ("attribution"; e.g., IPCC AR6 Seneviratne *et al.* 2021).

It is naïve to assume that any recent extreme event is caused by human influences on the climate. Climate is about the statistical properties of weather over decades, not single events. Further, there are only about 130 years of reliable observational records that can be analyzed statistically. That brief interval does not begin to contain all the extreme events that the climate system can create on its own. Over geologic time the climate system has generated an (essentially) infinite variety of weather patterns and extremes that humans have never observed and thus are absent from the databases used to determine extreme statistics [see *Perils of short data records* below]. For that reason, attributing an extreme event unprecedented in the record requires assumptions about the magnitude of natural variations.

This chapter is concerned with detection of trends in extreme weather, while Chapter 8 considers causal attribution, with Section 8.4 specifically addressing extreme weather. If no trend is detected, then clearly there is no basis for attribution. But even where a trend is observed, attribution to human-caused warming does not necessarily follow.

This is especially true for precipitation events. The hydrological literature has long noted the phenomenon of Long-Term Persistence (LTP) in rainfall data (Hurst 1951, Cohn and Lins 2005, Markonis and Koutsoyiannis 2016). LTP has a formal mathematical definition, but it can be thought of as the presence of natural variations with strong autocorrelations that result in cycle-like behavior on long time scales. The presence of LTP requires long records to accurately estimate variability. Analysis of records that are short relative to the LTP scale will tend to underestimate variability, therefore overstating the significance of apparent trends and underestimating the likelihood of extreme events (Cohn and Lins 2005).

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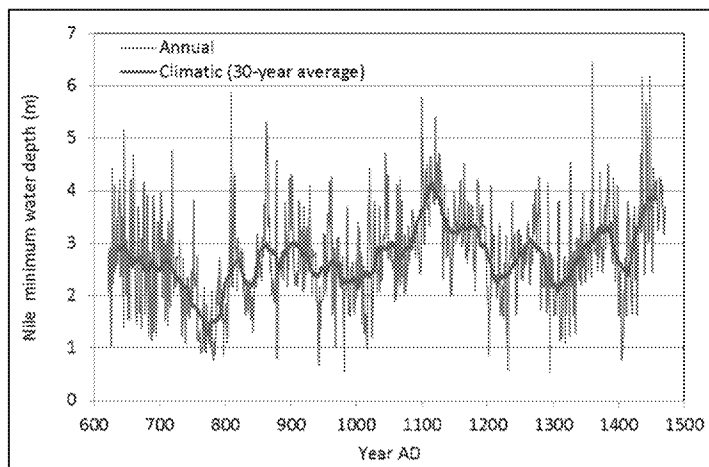


Figure 6.1.1: The annual minimum depth of the Nile River near Cairo over more than 650 years from 622 to 1284 A.D. The data, measured in meters, shows a characteristic pattern of year-to-year fluctuations around longer-term trends. Data from Koutsoyiannis (2013)

A good example of LTP is the eight-century-long record of the annual minimum height of the Nile River observed at Roda Island in Cairo shown in Figure 6.1.1. The Nile River is fed by precipitation over a 4 million square mile drainage basin, an area equal to about one third of CONUS. Since human influences on the global climate were negligible before the 20th century, the century-scale variability of the thirty-year average is entirely natural; Egyptians of the seventh and eight centuries would have been incorrect to assume that the worsening drought during that time was the “new normal.”

With these caveats in mind, we examine the evidence for changes in selected weather and climate extremes. A recurring theme is the wide gap between public perceptions and scientific evidence. It has become routine in media coverage, government and private sector discussions, and even in some academic literature to make generalized assertions that extreme weather of all types is getting worse due to GHGs and “climate change.” Yet expert assessments typically have not drawn such sweeping conclusions and instead have emphasized the difficulty both of identifying specific trends and establishing a causal connection with anthropogenic forcing.

In the sections to follow we provide excerpts from various IPCC and NCA assessment reports denoting the sources as follows:

SREX: The IPCC Special Report on Managing the Risks of Extreme Events and Disasters to Advance Climate Change Adaptation (2012)

AR6: The IPCC Sixth Assessment Report Working Group 1 (2021):

NCA4: The U.S. Climate Science Special Report of the Fourth National Climate Assessment (2017) Volume I:

NCA5: The U.S. Climate Science Special Report of the Fifth National Climate Assessment (2023) Volume I: [DON'T BELIEVE NCA5 WAS SPLIT INTO SEPARATE VOLUMES]

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The IPCC's AR6 provides the following assessment of tropical cyclones (TCs; used here as a synonym for hurricanes):

AR6: It is *likely* that the global proportion of major (Category 3–5) tropical cyclone occurrence has increased over the last four decades . . . There is *low confidence* in long-term (multi-decadal to centennial) trends in the frequency of all-category tropical cyclones. (IPCC, 2023 SPM p. 9)

Since 1980, when satellite observations fully covered the global oceans, we have confidence in the numbers of total global hurricanes and major hurricanes (Category 3 and higher). Figure 6.2.1 shows that on average, each year there are about 50 hurricanes each year, with about 25 reaching major hurricane status (Maue, 2025). There is substantial year-to-year and decadal variability, a weak decrease in the number of hurricanes, and a slight but insignificant increase in the number of major hurricanes. These two trends combine to create an overall increase in the proportion of major hurricanes.

Global hurricane statistics are dominated by the Northwest Pacific Ocean, which accounts for about 35 percent of total global hurricanes, whereas the Atlantic accounts for some 15 percent of global hurricanes (Colorado State University, 2025). Data in the Atlantic basin extends further back than in the other ocean basins and is most relevant to U.S. policy-makers.

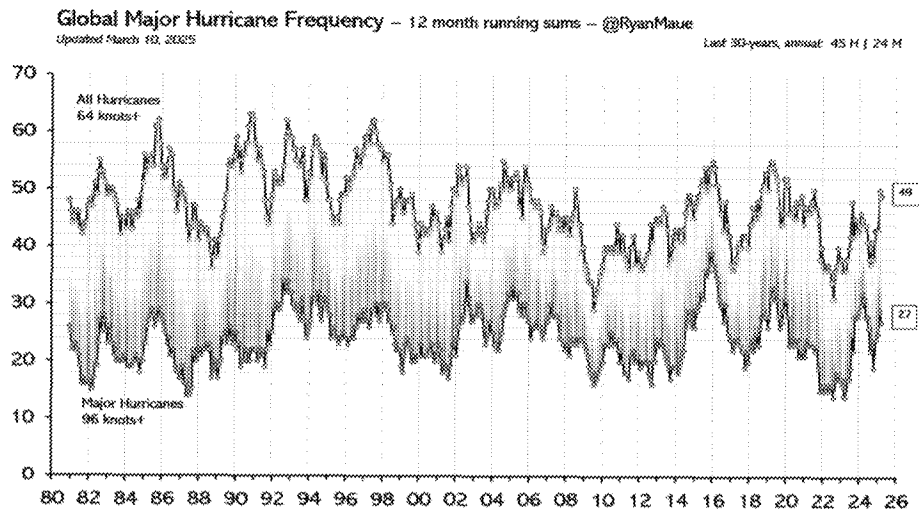


Figure 6.2.1: Global frequency of hurricanes and major hurricanes since 1980.

Figure 6.2.2 shows the frequency of North Atlantic hurricanes and major hurricanes (Category 3 and higher) back to 1920. Data prior to 1965 (the onset of satellite observations in the Atlantic, shaded in blue) shows some undercounting, with data prior to 1920 showing substantial undercounting (Vecchi and Knutson, 2011). All measures of Atlantic hurricane activity show a significant increase since 1970. However, the period from 1971-1994 saw exceptionally low activity, with high activity (comparable to the past two decades, even with undercounting) also observed during the 1950's and 1960's, and even in the 1930's.

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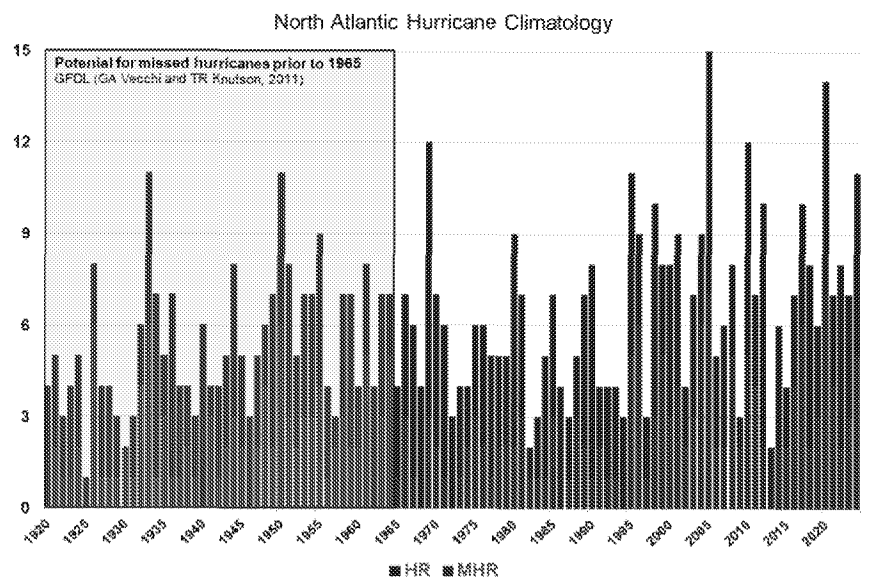


Figure 6.2.2: North Atlantic frequency of hurricanes (HR) and major hurricanes (MHR) since 1920. Source National Hurricane Center (2024)

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Figure 6.2.2 shows that Atlantic hurricanes vary strongly on decadal and multidecadal time scales. These variations are associated primarily with the Atlantic Multidecadal Oscillation (AMO), which is manifest in basin-wide sea surface temperature and sea level pressure fluctuations connected to large scale ocean circulation patterns. The AMO was in its warm phase during 1926-1970 and 1995-present, but in its cool phase during 1971-1994. It has its greatest impact on the number of major hurricanes (Category 3+), identified by Goldenberg *et al.* (2001) as associated with above normal SSTs and decreased vertical shear in the AMO warm phase (see also Bell and Chelliah, 2006; Klotzbach *et al.*, 2018).

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Klotzbach *et al.* (2018) conducted a comprehensive evaluation of the landfalling hurricane data for the Continental U.S. since 1900. Figure 6.2.3 updates their analysis through 2024. While the largest numbers of landfalling hurricanes are from 2004, 2005 and 2020, there is no statistically significant trend since 1920. Figure 6.2.3 also shows the time series for major hurricane landfalls (Category 3-5). The largest year in the record is 2005, with 4 major hurricane landfalls. However, following 2005 there were no major hurricanes striking the U.S. from 2006 through 2016, the longest such period since 1920.

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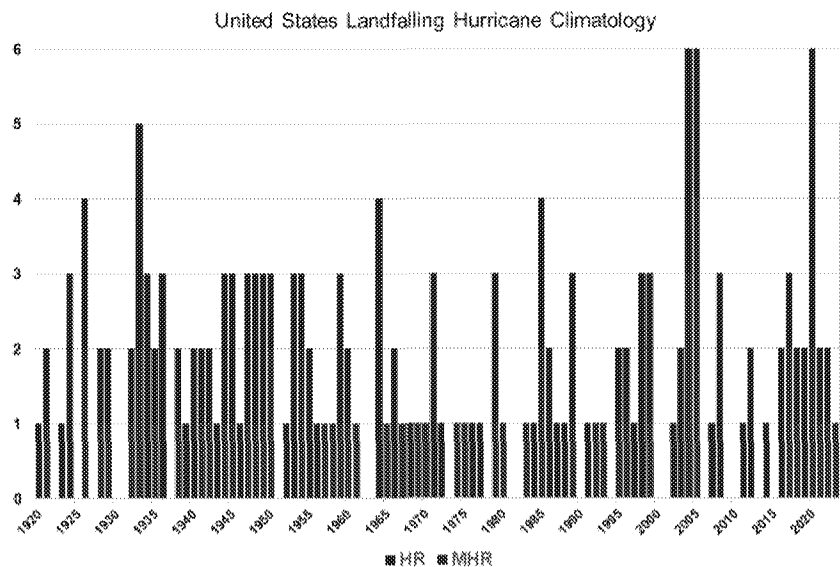


Figure 6.2.3: U.S. landfalling frequency of hurricanes (HR) and major hurricanes (MHR) since 1920. Source NOAA HRD(a) (2024)

Figure 6.2.3 shows substantial interannual to multidecadal variability in U.S. landfall activity. Klotzbach *et al.* (2018) examined how the landfall counts vary with ENSO (El Niño versus La Niña) and the warm versus cold phases of the Atlantic Multidecadal Oscillation (AMO).

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Villarini *et al.* (2012) provide an analysis of U.S. hurricane landfalls back to 1878. While it is possible that some landfalls were missed in the late 19th century owing to sparsely populated regions on the Gulf Coast, it is remarkable that the highest year in the entire record is 1886, with 7 hurricane landfalls, when human influences on the climate were much smaller than they are today.

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Table 6.2.1 shows the 10 strongest hurricanes (plus ties) to make U.S. landfall. Of the hurricanes that have made landfall with sustained winds greater than 150 mph, only one has occurred in the 21st century.

In summary, analyses of both global and regional variability and trends of hurricane activity provide the basis for detecting changes and understanding their causes. The relatively short historical record of hurricane activity, and the even shorter record from the satellite era, is not sufficient to assess whether recent hurricane activity is unusual relative to the background natural variability. Atlantic hurricane processes are influenced substantially by the natural modes of ocean circulation variability in the Atlantic, notably the Atlantic Multidecadal Oscillation. While it has long been hypothesized that a rising global sea surface temperature would cause an increase in hurricane intensity, identification of any significant trend in the hurricane data is hampered by a short data record and substantial natural variability.

Rank	Year	Landfall Wind (MPH)	Name
1	1935	185	"Labor Day"
2	1969	175	Camille
3	1992	165	Andrew
4	2018	160	Michael
5	1856	150	"Last Island"
5	1886	150	"Indianola"
5	1919	150	-----
5	1932	150	"Freeport"
5	2004	150	Charley
5	2020	150	Laura
5	2021	150	Ida
5	2022	150	Ian

Table 6.2.1 Strongest hurricanes to make landfall along the U.S. coast. Source (NOAA HRD(b), 2024)

6.3 Temperature extremes

The AR6 assessment focused on the period after 1950 and reported increasing trends in heatwave frequency and intensity. However, the NCA4 noted that heatwave activity in the U.S. reached a peak in the 1930s (Figure 6.3.1).

AR6: It is *virtually certain* that hot extremes (including heatwaves) have become more frequent and more intense across most land regions since the 1950s, while cold extremes (including cold waves) have become less frequent and less severe (SPM, A3.1)

AR6: In North America, there is very robust evidence for a *very likely* increase in the intensity and frequency of hot extremes and decrease in the intensity and frequency of cold extremes for the whole continent, though there are substantial spatial and seasonal variations in the trends. Minimum temperatures display warming consistently across the continent, while there are more contrasting trends in the annual maximum daily temperatures in parts of the USA. (Chapter 11, p 1550)

NCA4: Changes in warm extremes are more nuanced than changes in cold extremes. For instance, the warmest daily temperature of the year **increased** in some parts of the West over the past century, but there were **decreases** in almost all locations east of the Rocky Mountains. In fact, all eastern regions experienced a net **decrease**, most notably the Midwest (about 2.2°F [1.2°C]) and the Southeast (roughly 1.5°F [0.8°C]). (pp. 190-191)

NCA4: Since the mid-1960s, there has been only a very slight increase in the warmest daily temperature of the year (amidst large interannual variability). Heat waves (6-day periods with a maximum temperature above the 90th percentile for 1961–1990) increased in frequency until the mid-1930s, became considerably less common through the mid-1960s, and increased in frequency again thereafter. As with warm daily temperatures, **heat wave magnitude reached a maximum in the 1930s.** (pp. 190-191)

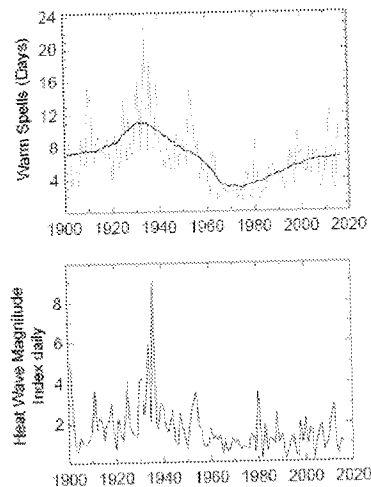


Figure 6.3.1: U.S. Heat waves since 1900. Source: NCA4 Figure 6.4

6.3.1 Temperatures in the U.S. are becoming less extreme

Daily maximum temperatures in the warm season (T max, May-Sep) and daily minimum temperatures in the cold season (T min, Dec-Mar) are available beginning in Dec 1898 (126 years). The dataset consists of 1,211 CONUS stations designated as United States Historical Climate Network or USHCN stations (see Figure 6.3.2; Quinlan *et al.* 1987, Karl *et al.* 1990). These stations were selected by NOAA as having the fewest problematic issues with gaps, station moves and instrument changes. Where gaps still exist, nearby stations (bias-corrected) were merged so that the median volume of data available for a station is 98%. Although there are certainly errors in the dataset, including unresolved spurious warming due to UHI effects that especially bias T min records, this data set is sufficiently accurate for assessing trends in T max heat extremes.

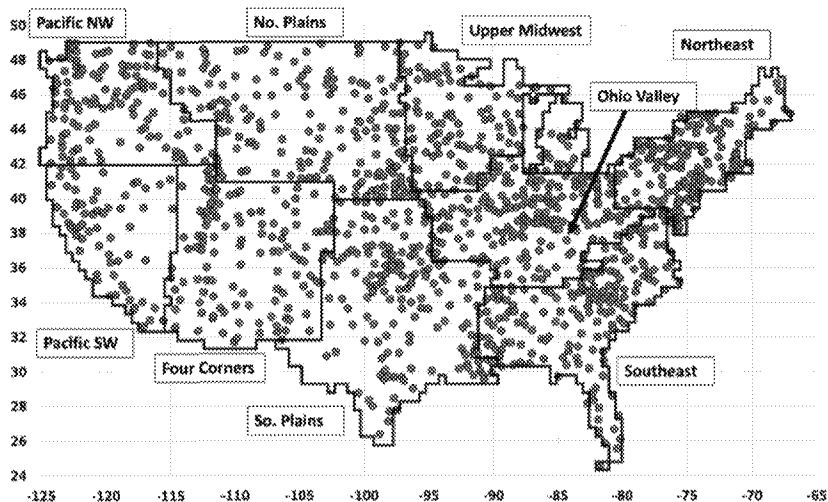


Figure 6.3.2 Locations of USHCN temperature stations. Source: USHCN.

We begin with the question of whether the occurrence of daily record high or low temperatures has changed since Dec 1898. Each warm-season has 153 days (1 May to 30 Sep) and each cold-season has 122 days (1 Dec to 31 Mar). For each station and day, we calculated the year in which the record highest (lowest) temperature occurred. With 126 years of observations, if there were no temperature trends over time, the expected number of records for Tmax would be 1.21 ($=153/126$) per station per year and for Tmin 0.96 ($=122/126$) per station per year.

Figure 6.3.3 shows the observed distribution in time of the occurrence of these extreme events. There is a common feature in many metrics of warm-season extremes in the CONUS - the exceptional heat of the 1920s and especially the 1930s, peaking in 1936. On a per-station average, 60 percent of the Tmax records and 59 percent of the Tmin records occurred in the first half of the period (1899-1961).

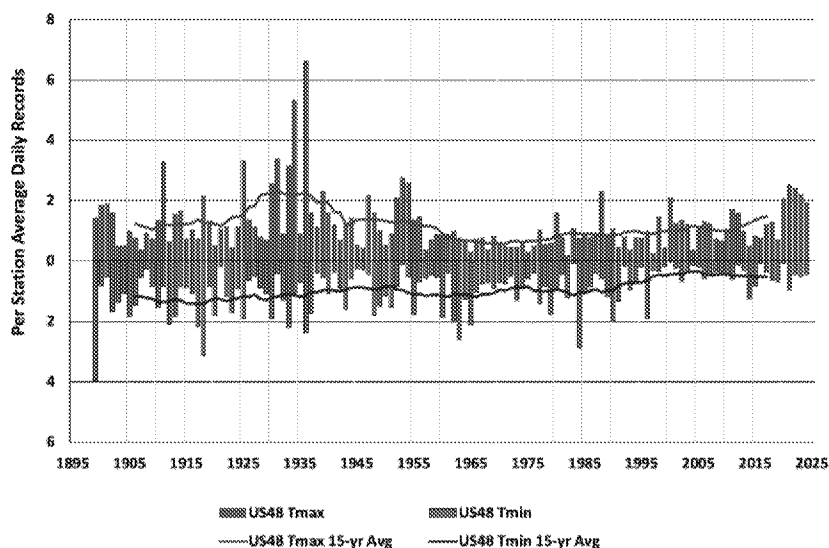


Figure 6.3.3 Number of daily record High (red) and Low (blue) temperatures for warm and cold seasons in the CONUS. The lines represent the 15-year running, centered average. Source: 1,211 USHCN stations supplemented as needed to achieve a minimum of 92 percent of observations in the 126-year period since Dec 1898. US48: contiguous U.S. states. Tmax: maximum temperature. Tmin: minimum temperature.

On the cold side, the Valentine's Day Arctic outbreak in Feb 1899 stands as the most extensive cold extreme experienced by CONUS, with 1917 in 2nd place. The frequency of cold records has declined, especially over the last quarter of the period in which only 13 percent of the extreme cold events were measured. In contrast, 25 percent of the extreme Tmax records were achieved in the last quarter, in accordance with statistical expectations. These general features have been noted in past assessments (see above, IPCC AR6, NCA4). Combining the two records, the overall reduction in numbers of both cold and hot extremes over the past century indicates a climate less prone to extremes.

This pattern is also shown in Figure 6.3.4. For each station and for each year the hottest warm season and coldest cold-season temperatures were calculated. Then the differences between these were computed by station and geographically-averaged over all stations, thus yielding an annual measure of the expected range of local extreme temperatures for each year. Figure 6.3.4 shows the 15-year trailing average of this measure, which has clearly declined over the past century.

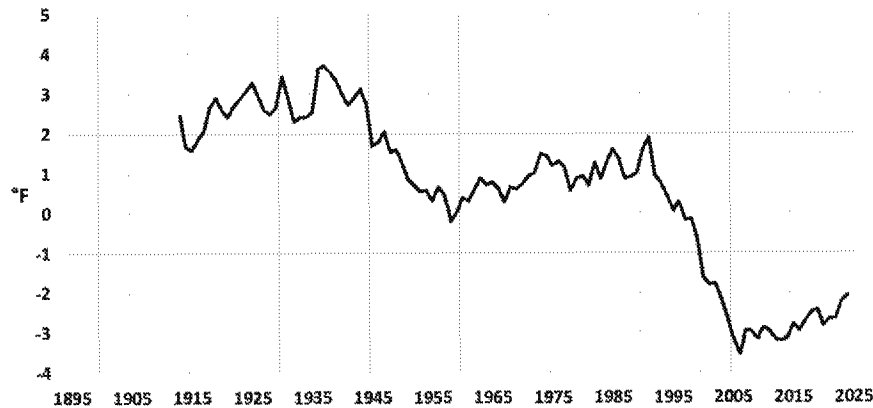


Figure 6.3.4. 15-year trailing average of the difference each year of each station's hottest warm-season Tmax and coldest cold-season Tmin relative to the long-term average. Source: Author analysis of USHCN data.

The average difference for each station between the hottest summer Tmax and coldest winter Tmin has declined by about 5°F in the past 126 years. The decline is due mostly to warmer winter Tmin, but a decline cooler in summer Tmax is also a factor. The rise in Tmin has been strongly related to the growing presence of manufactured surfaces around the weather stations over the last 100+ years (the so-called urban heat island effect; [Section 3.3](#) and [Spencer et al. 2025](#)).

In summary, while temperature extremes are regularly experienced in the U.S. and attract a great deal of media attention, long term records show the U.S. climate has become less extreme over time (milder) when measured by the range between warm season maxima and cold season minima.

6.3.2 Exceedances of a heat threshold

Under the heading of “The Risk of Temperature Extremes is Changing”, the most recent U.S. National Climate Assessment report (NCA5) notes the increase in a threshold metric of number of days at or above 95°F, stating,

The western U.S. has been particularly affected by extreme heat since the 1980s ..., experiencing a larger increase in days over 95°F, as would be expected given the greater warming in that region relative to the eastern US. Several major heatwaves have affected the U.S. since 2018, including a record-shattering event in the Pacific Northwest in 2021.

Are the occurrences of 95°F days changing? In a climate as varied as that of CONUS, threshold statistics can be misleading. A region with many stations that have near 95°F Tmax average temperatures in the summer might see large swings in the metric when only small changes in average temperature occur. Elsewhere with stations that either rarely or virtually always achieve 95°F Tmax temperatures, a small change will not have much impact on the results.

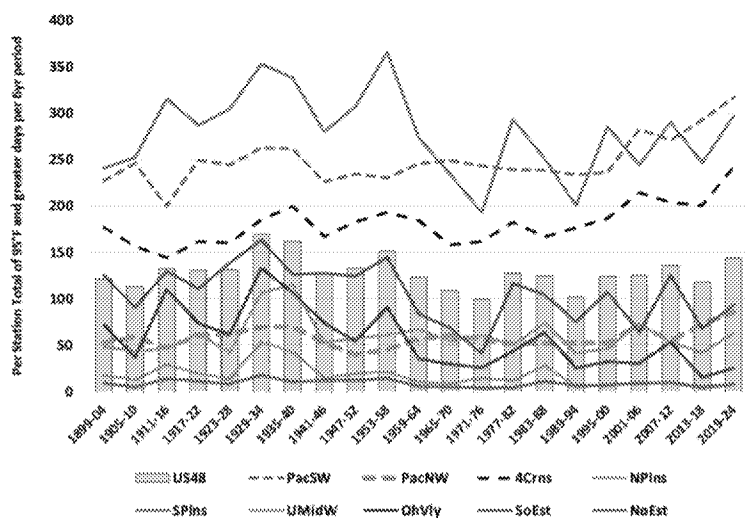


Figure 6.3.5. Total number of days $\geq 95^{\circ}\text{F}$ in 6-yr periods, U.S. 48 (bars) and regions (lines). A 6-year period is used as this evenly divides the 126-year record. US48: contiguous U.S. states. See Figure 6.3.2 for region names. Source: Author analysis of USHCN data.

In the past 126 years, the average CONUS station experienced 129 days exceeding 95°C per 6-yr period, but the regional values range from 278 in the Southern Plains to 9 in the Northeast. Thus, such threshold analyses must be interpreted with caution. Figure 6.3.5 shows that only three of the nine regions, all in the West, have experienced upward trends in the number of 95°F or hotter days (dashed lines). The CONUS as a whole has not, and the other six regions have experienced declines.

The Pacific NW heatwave of 2021 referenced in the NCA5 quote will be examined more closely in Section 8.6.1. The evidence indicates that it was a single, unprecedented event in the record, not part of a pattern of increasing extreme heat. For example, the 5-day average tropospheric grid-point temperature anomaly over the Pacific NW during that event was $+10.8^{\circ}\text{C}$, the most extreme Northern Hemisphere grid point summer anomaly in the 46 years from over 4 million grid values. In contrast, the global temperature anomaly during that time was virtually zero ($+0.03^{\circ}\text{C}$, Mass *et al.* 2024).

The NCA5 heading “The Risk of Temperature Extremes is Changing” suggests pervasive positive trends are now being observed in threshold temperatures, but that is not evident in the figures above.

6.3.3 Heatwaves

Heatwaves (consecutive days that exceed an extreme threshold) have a greater societal impact than a single daily record temperature. We measure “Heatwave Days” here as the count of all days in May-Sep each year that exceed the 90th percentile for that day and that lie within a period of at least six consecutive days. This is equivalent to the method used in NCA4, except that the reference period here is the entire record 1899-2024, while NCA4 truncated the reference period to 1961-1990, which was a cool period. That truncation boosts positive results (days exceeding the 90th percentile) in periods warmer than the reference period, especially starting in 1960 and moving to the present (see Figure 6.3.7 and discussion below).

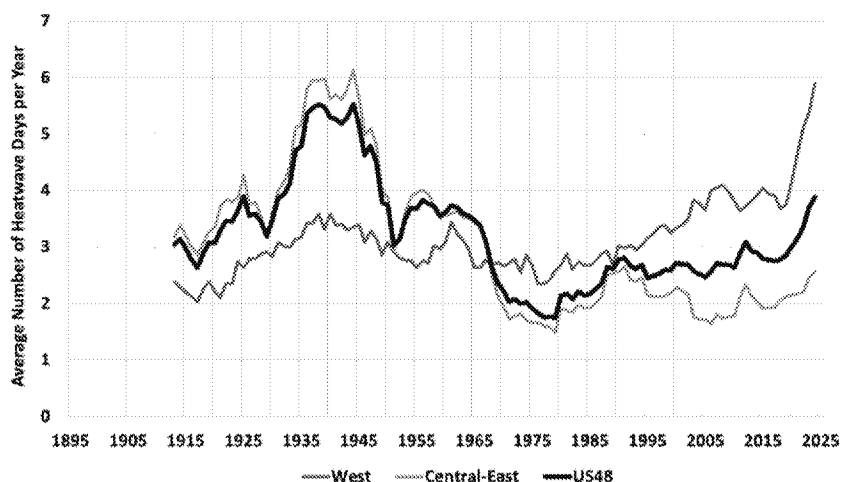


Figure 6.3.6 15-year trailing average of number of heatwave days per year per station in the CONUS (blackline) and two regions: West (red), Central-east (green).

Figure 6.3.6 indicates that there are regional variations in heatwave activity. The excessive heat of the first half of the 20th century occurred primarily in the eastern two-thirds of the nation, while the West has seen a recent increase of heatwave days (NCA5). This indicates that the background warm season circulation favored heatwaves in the eastern portions of the country in the first half of the 20th century, but in the 21st century the patterns have favored heatwaves in the West. For CONUS as a whole, heatwaves are no more common today than they were a century ago, consistent with the upper panel of our Figure 6.4.1 taken from NCA4.

This metric varies significantly with region. The four northern regions (Pacific NW, Northern Plains, Upper Midwest, and Northeast) on average experience 15 to 27 heatwave days per 15-year period. In contrast, the five southern regions (Pacific Southwest, 4-Corners, Southern Plains, Ohio Valley and Southeast) see 37 to 54 such days, essentially twice as many. This suggests the summer circulation pattern is more prone to stationary events in the southern regions while transient systems in the northern regions are more common and thus cut short these potentially longer events.

The analysis of heatwaves is an example of why it is important to consider complete datasets and appropriate metrics. The NCA5 directs readers to the website [[HYPERLINK "https://www.globalchange.gov/indicators/heat-waves"](https://www.globalchange.gov/indicators/heat-waves)] (USGCRP 2023) to view a figure showing the number of urban heatwaves by decade from the 1960s, which we reproduce as Figure 6.3.7.

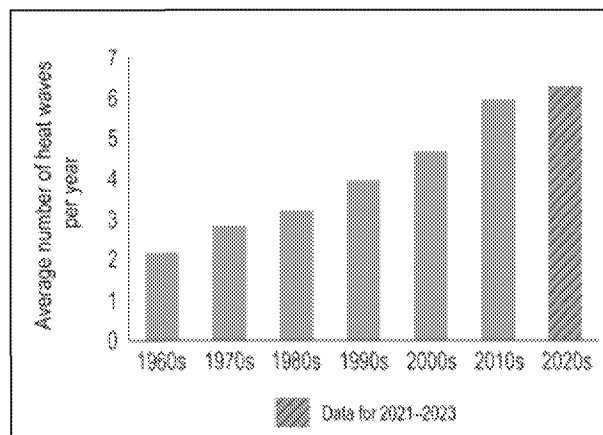


Figure 6.3.7: Average number of urban heatwaves per year for 50 large U.S. metropolitan areas, a misleading metric for reasons explained in the text. From [[HYPERLINK "https://www.globalchange.gov/indicators/heat-waves"](https://www.globalchange.gov/indicators/heat-waves)]

The figure shows a monotonic increase in each decade from 2 occurrences per year in the 1960s to 6 in the 2020s. The definition of a heatwave used is an unusual but practical measure of human discomfort - a period of at least 2 consecutive days when the *minimum* apparent temperature (combination of temperature and humidity) exceeds the 85th percentile. Note too, the dataset is limited to the 50 largest U.S. cities.

Given the unusual heatwave definition and urban focus, these increasing values since 1960 presented in USGCRP (2023) are not informative about long term trends or the influence of GHG emissions for at least two reasons. First, as shown in Figures 6.3.1 and 6.3.6, the 1960s was the coldest decade and 1970s the second coldest decade since the 1910's, so this starting date preconditions the time series to show increases. Second, post-1960 urbanization in these cities is a major factor in the rise of T_{min} relative to co-located T_{max} and relative to T_{min} at nearby rural stations. This does not dismiss the real rise in nighttime temperatures in major U. S. cities and the societal impacts associated with these changes. However, we note for a variety of reasons, that summer T_{max} (especially in rural areas) is a better metric for detecting changes in heatwaves influenced by changes in the background climate due, for example, to increasing GHGs (Christy *et al.* 2009). For the CONUS as a whole, the evidence in this section suggests GHG emissions have had little-to-no effect on heatwaves.

6.4 Extreme precipitation

The IPCC AR6 assessed that an increase in heavy precipitation has been observed in data starting in the 1950s.

AR6: The frequency and intensity of **heavy precipitation events have increased** since the 1950s over most land area for which observational data are sufficient for trend analysis (high confidence). (SPM A3.2)

AR6: In North America, there is robust evidence that the **magnitude and intensity of extreme precipitation has very likely increased** since the 1950s. Both [one-day maxima] and [5-day maxima] have significantly increased in North America during 1950-2018. (Chapter 11, p. 1560)

The U.S. National Climate Assessments (NCA4, NCA5) have highlighted an increase in the occurrence of the heaviest precipitation events (defined in different ways) primarily in the eastern half of the CONUS, especially the Northeast, when starting the analysis in either 1901 or 1958. Interestingly, the regional variations indicate that the largest increases in extreme precipitation events are in the Northeast and the smallest in the West, a pattern counter to the changes in temperature extremes.

McKittrick and Christy (2019) examined long-term and consistent station observations of extreme daily precipitation to test some of these NCA claims for the Southeast and West Coast using a trend model with a non-parametric variance estimator robust to the complex autocorrelation properties of precipitation data. When the time series were extended back in time (as far as 1872 in some cases) or when starting later (1978), there were no significant trends for either region.

These findings have been updated for this report (McKittrick and Christy 2025) with similarly constructed observations from 29 stations on the CONUS Pacific Coast (1893ff from San Diego CA to Blaine WA) and 24 stations in the humid Southeast (1872ff from Austin TX to Washington DC), also adding 27 stations in the Northeast (1888ff from Buffalo NY to Eastport ME). The locations are shown in Figure 6.4.1. The stations were selected based on availability of long-term high-quality records. The regions are each associated with important features of extreme precipitation behavior: the Pacific Coast is associated with landfalling atmospheric rivers for which AR6 cites evidence of increasing activity since 1948 with further increases expected as the world warms (AR6 8.3.2.8.2); the NCA report indicates that the Northeast has experienced the greatest increase in extreme events, and the Southeast is also a place noted in the NCAs as having increased extreme events.

The results of applying the analysis of McKittrick and Christy (2019) were as follows for each region, then followed by further explanation.

Pacific Coast heavy rainfall events

- The average precipitation trend is statistically significant (downwards) in Astoria OR; insignificant elsewhere.
- The trend in rainfall variance is positive and significant in Big Sur CA; insignificant elsewhere.
- The trend in daily maximum precipitation is positive and significant in Aberdeen WA and Big Sur CA and negative and significant in Newport OR (insignificant elsewhere).
- Averaged over all stations in the region, none of these three trend parameters is statistically significant.

The Pacific Coast receives considerable precipitation from Atmospheric River (AR) events, which often last more than a day or two (e.g., Gershunov *et al.* 2017, Pan *et al.* 2024). The worst series of such events in recent history was the so-called ARkStorm that occurred between December 1861 and January 1862; it dumped nearly 10 feet of rain in parts of California and submerged the entire Central Valley for weeks under as much as 15 feet of water (Brewer 1930, Null and Hulbert 2007). Additionally, paleoclimate research has found six megastorms more severe than 1861–1862 in California during the last 1800 years, occurring at intervals of 300 years or so (Porter *et al.* 2011).



Figure 6.4.1: Locations of precipitation monitoring stations used in this report. Orange: Pacific Coast. Blue: Northeast. Green: Southeast. Data from McKittrick and Christy (2025).

We examine occurrences of 5-day deluges as follows. Taking the Pacific Coast as an example, a 130-year span contains 26 5-year intervals. At each location we computed the 5-day precipitation totals throughout the year and selected the 26 highest values across the sample. A single year may have more than one of the 26 heaviest. Each of those can be thought of as 1-in-5-year events. If there are no trends in precipitation, then the total number of these events across all stations should be evenly spread out distributed over the years. In Fig. 6.4.2 we show the distribution in time of these events for the Pacific Coast. The deluges associated with the massive 1997-98 El Niño event are readily apparent. While erratic, as is typical of such precipitation metrics, there is no indication of a tendency to become more frequent over time.

Southeast heavy rainfall events

- The trend in average precipitation is positive and statistically significant in Mobile AL and Quitman GA but insignificant elsewhere.
- The trend in rainfall variance is positive and significant in Mobile AL but insignificant elsewhere.
- The trend in daily maximum precipitation is positive and significant in Vicksburg MS and Norfolk VA but insignificant elsewhere.
- Averaged over all stations in the region none of these three trend parameters is statistically significant

Figure 6.4.3 shows the same calculation for the last 150 years in the Southeast humid zone. The temporal pattern of 5-day totals of the 1-in-5-year heavy events is generally unremarkable, though a cluster of higher values appears in 1995 to 2019. The increase in those years is due largely to the ~~4~~four northeastern-most stations of Wilmington NC, Weldon NC, Washington DC, and Norfolk VA. This confirms the pattern indicated in NCA4 and NCA5 -- an increasing frequency of heavy events due to a temporal clustering of tropical storms from eastern NC to Maine discussed below. Otherwise, the remaining 20 stations show an unremarkable temporal distribution of heavy events.

BOX: Perils of short data records

San Francisco provides a good case study of the limitation of using short historical samples to characterize natural variability of extreme events. Suppose we use a 130-year sample of daily San Francisco precipitation from 1895 to 2024 and we look for 3-day, 5-day, 14-day and 30-day rainfall records. The results are as shown in Table 6.2.

Event	Record (inches)	Year
3-day	6.94	2023
5-day	8.55	2023
14-day	12.62	2023
30-day	18.93	1998

Table 6.2: Extreme rainfall records, San Francisco, 1895-2024.

The records all cluster in the more recent years. 2023 appears to be an exceptional year and since it is near the end of the sample, it might suggest that the climate has shifted into a more hazardous state, perhaps because of human influences. Note: “2023” indicates the event occurred in the water-year of Aug 2022 to Jul 2023.

But the picture is very different if we use a sample that begins 45 years earlier, in 1850. Table 6.3 shows that the record-setting events all happened in the 1860s. Furthermore 2023 is now not even in 2nd place but falls to 3rd or 4th place. And comparing the records of the two charts shows the extreme precipitation events in 1862 and 1867 involved considerably more rainfall than the 1998 and 2023 events, with 14- and 30-day totals about 50 percent higher.

Event	Record (inches)	Year	Rank of 130-yr extreme listed in Table 6.2
3-day	8.85	1867	3
5-day	9.80	1867	3
14-day	19.05	1862	4
30-day	28.25	1862	2

Table 6.3: Extreme rainfall records, San Francisco, 1850-2024.

The range of natural variability is made even more remarkable when paleoclimate evidence is examined. Porter *et al.* (2011) discovered that in the past 1,800 years at least six megastorms were more intense than the devastating 1861-62 ARKStorm that struck the region. Evidently such extreme events, “unprecedented” in our 1895-2024 sample, have impacted the region about every 300 years, though not since 1895.

This example illustrates the limitations of using relatively short climate periods (~130 years) to assess the character and range of natural variability in general and of extreme events in particular. Accurate representation of the full range of natural variability is necessary for any attribution analyses (see Section 8.6). Infrastructure planners, emergency management institutions and attribution scientists would understand the significant mischaracterization of the magnitude of a future extreme if based only on the last 130 years. In this case, a single time-sample of 130 years provides an underestimation of the extreme value by up to 50 percent determined when adding only 45 more years of observations. Compared to millennial-scale paleoclimate evidence, an even greater underestimation would occur. An important lesson is that the climate can deliver great surprises on its own, even without human influences.

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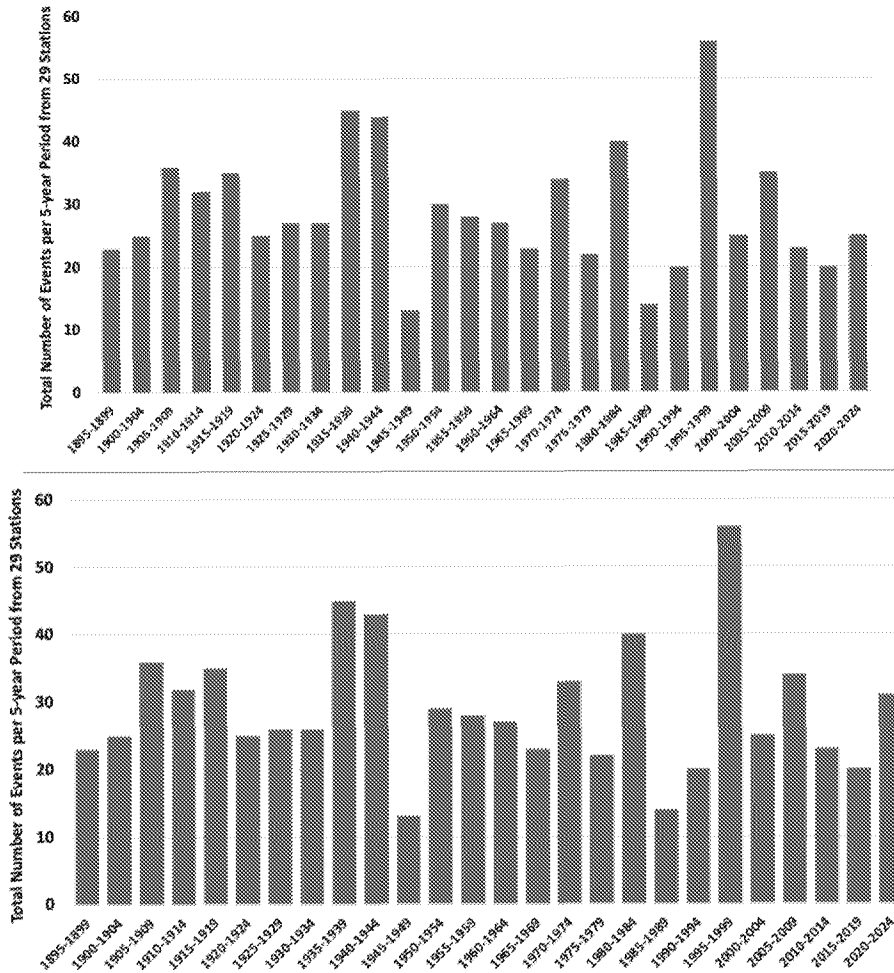


Figure 6.4.2. The time distribution by 5-year periods of the 26 heaviest (1-in-5-year) occurrences for 29 stations on the Pacific Coast.

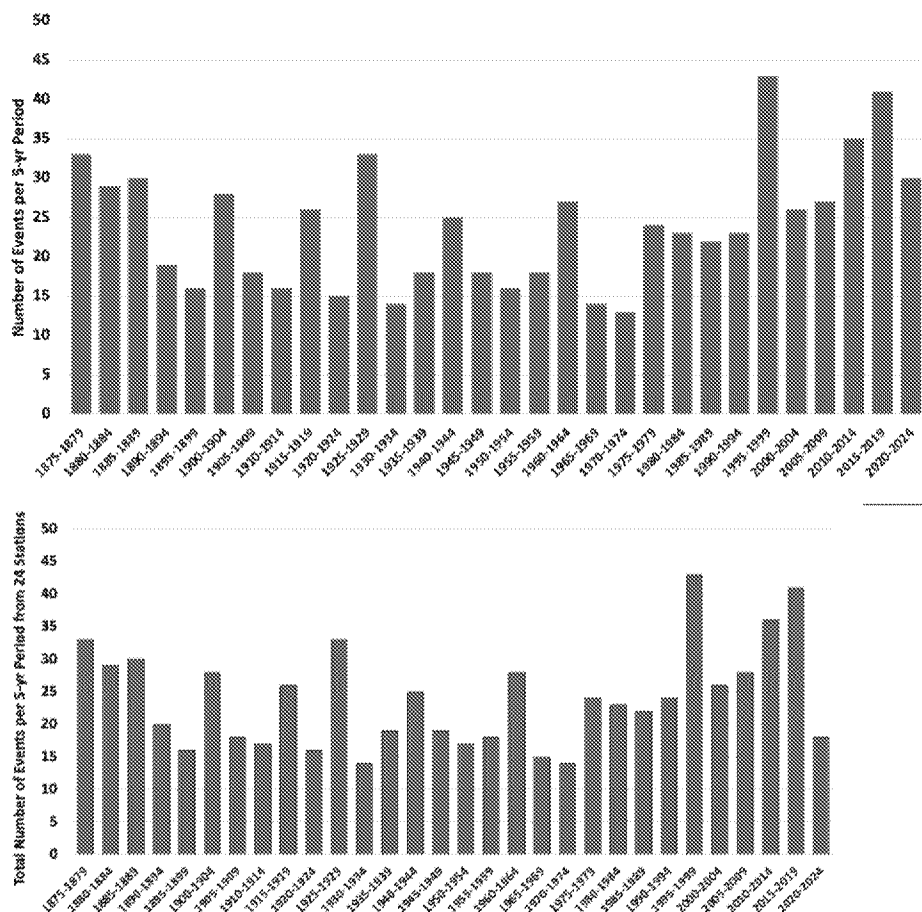
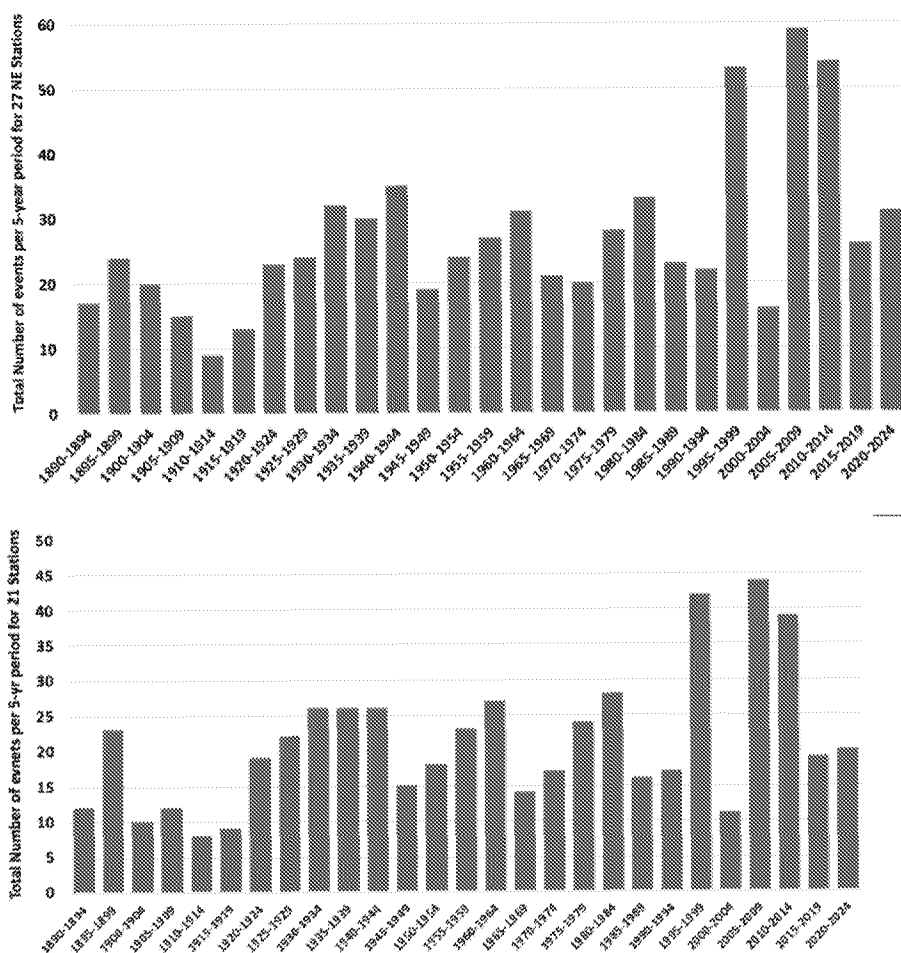


Figure 6.4.3. As in Fig. 6.4.2 but for the heaviest 30 (1-in-5-yr) events for 24 stations in the humid Southeast from Austin TX to Washington DC in 5-year bins for 1875-2024.



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Figure 6.4.4. As in Fig. 6.4.2 but for the heaviest 27 (1-in-5yr) 3-day precipitation events for 27 Northeast stations from NY to ME, including Montreal.

Northeast heavy rainfall events

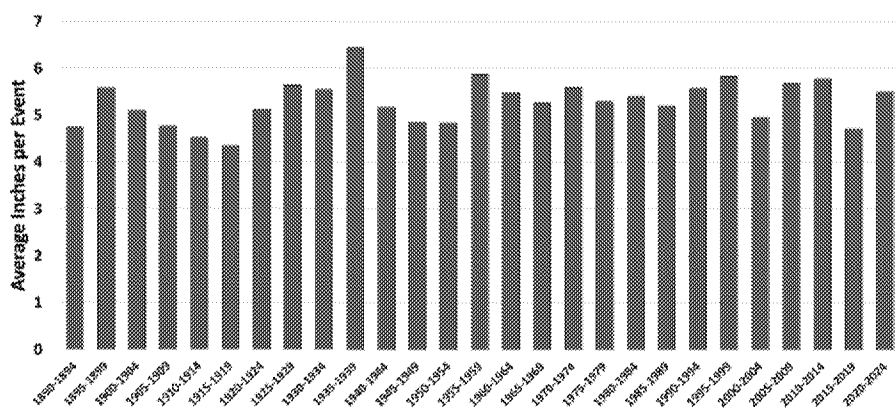
- The trend in average precipitation is positive and statistically significant in 12 of 27 locations and also in the regional average.
- The trend in rainfall variance is positive and significant in Portland ME, Albany NY, Buffalo NY and Eastport ME but insignificant elsewhere.
- The trend in daily maximum precipitation is positive and significant in Portland ME, Gardiner ME and Eastport ME but insignificant elsewhere.

- When averaged over all stations in the region, there is no statistically significant trend in either the precipitation variance or maximum

Fig. 6.4.4 shows the same calculation for the last 135 years in 27 stations in the Northeast (including Montreal Canada). We use 3-day totals here as this produced the largest temporal variations in time. In this region, 77± percent of events occur during June to October and are dominated by incursions of hurricanes, tropical storms, or tropical storms that transition to extratropical systems. According to NCA4 and NCA5, this region experienced the largest increases in extreme events, so it merits a closer examination.

There is a noticeable clustering of extreme events from 1995 to 2014. Howarth *et al.* (2019) examined a similar region as in Fig. 6.5.5 that includes PA and NJ and reported significant differences in various precipitation extremes between two 18-year periods, 1979-96 and 1997-2014. That included a 317 percent increase in 24-hr events exceeding 6 inches, while we find a 58± percent increase over the same years. However, Figure 6.5.5 shows that frequency drops sharply after 2014, returning to the long-term average in the subsequent 5-year intervals, again illustrating the perils of drawing conclusions from short-term trends in highly variable metrics.

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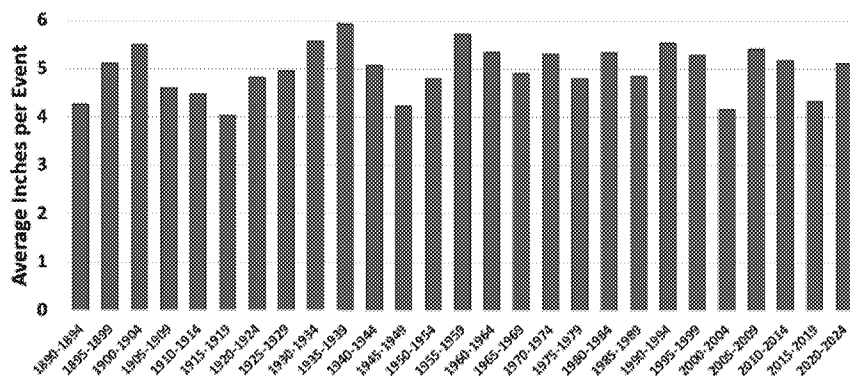


Figure 6.4.5. Average amount of precipitation falling in the 1-in-5-year events for the NE stations.

The high percentage increase in the Howarth *et al.* sample is associated with small numbers at relatively few locations: there were only six in the first period and 25 in the second across 58 stations. Most of the stations did not record such an event. If there is a region-wide increase in heavy events, it should be seen in the average across all stations. Figure 6.4.5 shows the average precipitation in the 1-in-5-year events across the NE. The trend is only +0.042 inches/decade. The highest amount, 1935-39, includes the Great New England Hurricane of 1938, one of the rare major (Category 3 or higher) hurricanes to strike the region. The results in Figs. 6.4.4 and 6.4.5 thus suggest that though there was a surge in the number of events at a few locations during the 1995 to 2014 interval, there was no regional pattern and the change did not persist beyond 2014.

Jong *et al.* (2024) document the increase of tropical influences on precipitation events in the Northeast since 1959 and concluded, “The autumn extreme precipitation trend over the Northeast U.S. is primarily attributed to tropical cyclone-related events since the 1990s.” The question then becomes: Was the temporal clustering of tropical systems in 1997-2014 which affected the Northeast a response to increasing GHGs? Jong *et al.* examined CMIP-6 model output which that suggests that there will be fewer such systems in the 21st century but that the intensity of the rainfall events might increase. This conjecture is not seen insupported by Fig. 6.4.5, where the amount-per-event has remained steady over the 135-year period.

There is some evidence to indicate the heaviest rainfall events may might be redistributed due to the impact of urban infrastructure on the local weather (e.g., Pielke Sr. *et al.* 2011, Zhang *et al.* 2018, Yang *et al.* 2024). Yang *et al.* state “Cities that experience compact development tend to witness larger increases in extreme rainfall frequency over downtown than their rural surroundings, while the anomalies in extreme rainfall frequency diminish for cities with dispersed development.” While this is an important insight to consider, the effect on the specific stations used in this analysis is unknown, or at least not detectable in Fig. 6.4.5.

In summary, some U.S. regions show short-duration increases in extreme precipitation events, consistent with natural variability. But analysis of long term, nationwide historical records that considers the autocorrelation properties of precipitation data does not support the claim that extreme short-duration rainfall events are becoming more frequent or intense.

6.5 Tornadoes

The IPCC AR6 assesses tornado trends in the U.S. as follows:

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[O]bservational trends in tornadoes, hail, and lightning associated with severe convective storms are not robustly detected due to insufficient coverage of the long-term observations. There is *medium confidence* that the mean annual number of tornadoes in the USA has remained relatively constant. (Chapter 11, section 11.7.3, p. 1594)

The monitoring of weak tornadoes has changed over time. The growth of rural populations and the increasing ability to take video with hand-held devices has led to more frequent reports of weak tornadoes that produce minimal damage. In contrast, strong to violent tornadoes have been observed more consistently over time. Note that tornado strength is measured by the damage it produces, not by the visual appearance of the funnel. Limited real-time observational capabilities in earlier decades did not prevent identification because strong to violent tornadoes leave much more damage which will be assessed later even if the tornado itself was not observed. Since statistics began in 1950, there has been a substantial decrease (by about 50%) in the number of strong to violent tornadoes as shown in Fig. 6.5.1a.

To summarize, there is a noticeable downward trend in the number of severe tornadoes in the U.S. since 1950. After 1990 the number of weak tornadoes in the U.S. has remained roughly constant; data before that are incomplete due to limited monitoring.

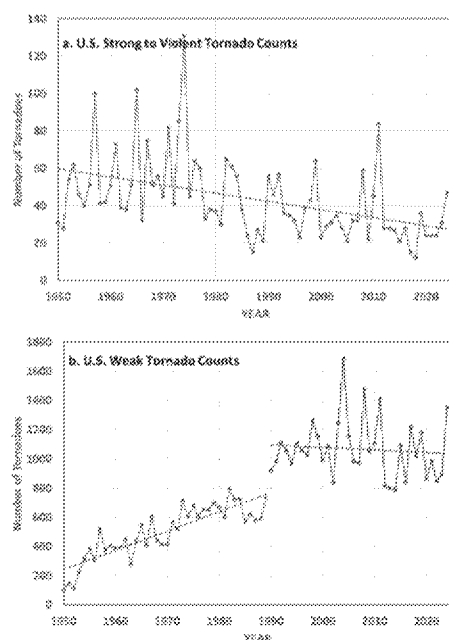


Figure 6.5.1. Annual U.S. tornado counts for (a) strong to violent tornadoes (EF3 to EF5), and (b) weak tornadoes (EF0 to EF2). Based upon NOAA Storms Prediction Center data, available at https://www.spc.noaa.gov/wcm/data/1950-2024_actual_tornadoes.csv

6.6 Flooding

Changes in floods were assessed as follows:

AR6: The SREX **assessed low confidence for observed changes in the magnitude or frequency of floods** at the global scale. This assessment was confirmed by the AR5 report. The SR15 found increases in flood frequency and extreme streamflow in some regions, but decreases in other regions. . . [H]ydrological literature on observed flood changes is heterogeneous, focusing at regional and sub-regional basin scales, making it difficult to synthesise at the global and sometimes regional scales. (Chapter 11.5)

AR6: [T]he seasonality of floods has changed in cold regions where snowmelt dominates the flow regime in response to warming (*high confidence*). Confidence about peak flow trends over past decades on the global scale is *low*. (Chapter 11.5)

NCA4: Trends in extreme high values of streamflow are **mixed** across the United States. Analysis of 200 U.S. stream gauges indicates areas of both increasing and decreasing flooding magnitude **but does not provide robust evidence** that these trends are attributable to human influences (pp. 240-241)

The absence of detectable US-wide trends in flooding is consistent with the findings in Section 6.4 of absence of coherent changes in extreme precipitation.

6.7 Droughts

Assessments of drought trends were as follows.

AR6: Few AR6 regions show observed increases in meteorological drought (Section 11.9, p. 1575),

AR6: Increasing trends in agricultural and ecological droughts have been observed on all continents (*medium confidence*), but decreases only in one AR6 region (*medium confidence*). Increasing trends in hydrological droughts have been observed in a few AR6 regions. (Chapter 11 Summary)

NCA4: As a consequence of this increased precipitation, **drought statistics over the entire CONUS have declined**. (p. 233)

NCA4: Recent droughts and associated heat waves have reached record intensity in some regions of the United States; however, by geographical scale and duration, the Dust Bowl era of the **1930s remains the benchmark** drought and extreme heat event in the historical record (very high confidence). (p. 231)

SREX: From a paleoclimate perspective, **recent droughts are not unprecedented**, with severe ‘megadroughts’ reported in the paleoclimatic record for Europe, North America, and Australia. (p. 170)

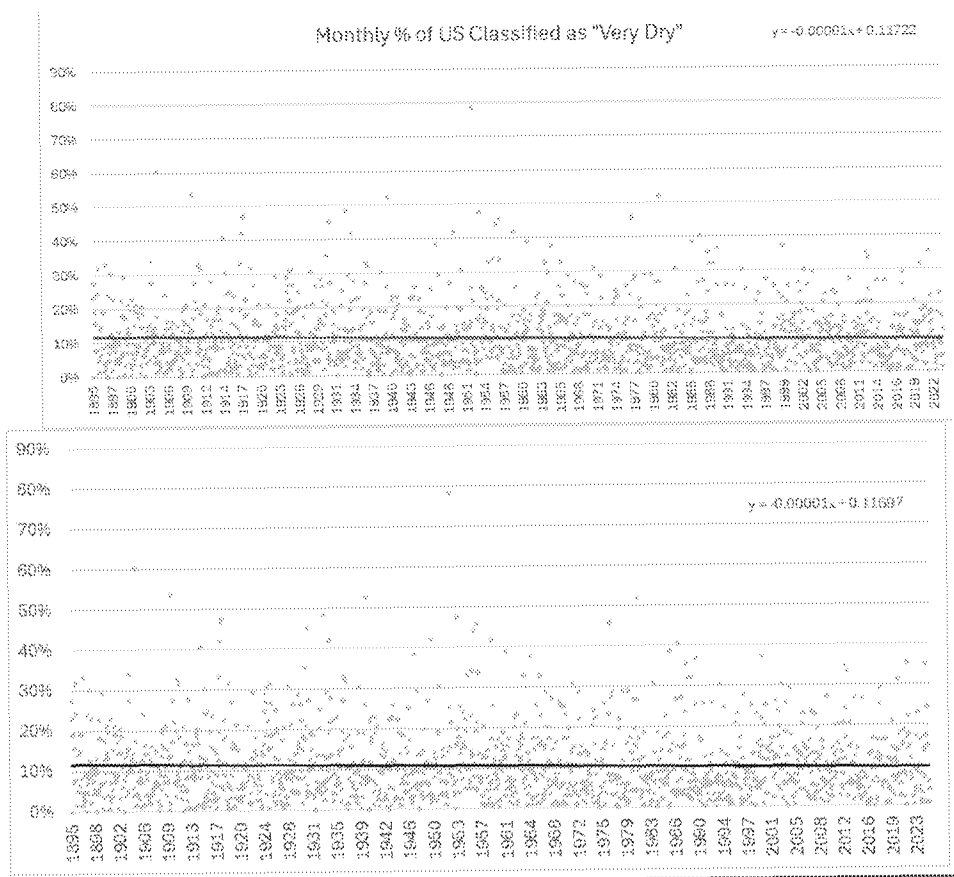


Figure 6.7.1: Monthly percent of US U.S. dryness (%) classified as "Very Dry" 1895–2023/2025.
 Source: Data source: NOAA [HYPERLINK "<https://www.ncsl.noaa.gov/access/monitoring/uspa/wet-dry/0>"] least squares trendline added.

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As shown in Figure 6.7.1, U.S. long-term data shows an insignificantly declining trend in extreme dryness (-0.001 percent per year)

Kogan *et al.* (2020) examines a 38-year high-resolution satellite-based drought measure and concludes that global drought has not intensified and is not connected to climate change: "it is possible to state firmly that global and main grain countries' drought area and intensity trends have not been following global climate warming since 1980's."

In summary there is no evidence of increasing drought frequency or intensity in the U.S. or globally over recent decades.

6.8 Wildfires

The IPCC has not provided an assessment of wildfires. As shown in Figure 6.8.1, global wildfire activity as measured by European Space Agency shows a downward trend in the 21st century.

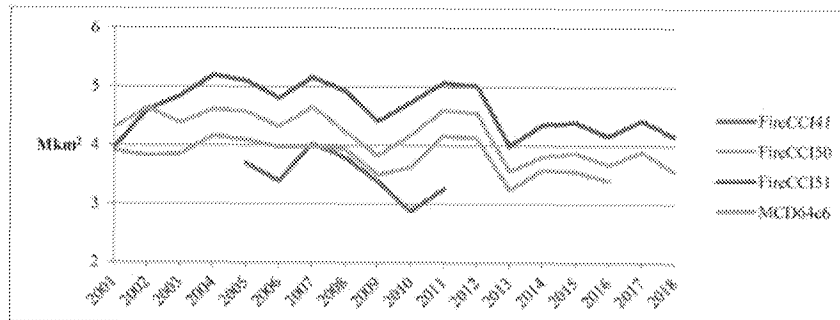


Figure 6.8.1: Global wildfire area 2001-2018. Source: From Lizundia-Loiola *et al.* (2021) Figure 12. The different coloured lines represent data products derived from different satellites and algorithms

Global data show that wildfire coverage is constant or declining on every continent (Samborska and Ritchie, 2024).

Active fire suppression since 1900 makes it difficult to establish a natural baseline for wildfire activity in the U.S. Paleoclimatic evidence indicates that past activity was much higher than today. Marlon *et al.* (2012) used sedimentary charcoal layers to reconstruct fire history of western U.S. for the past 1400 years and also fit a model to predict fire activity as a function of climatic conditions. Their results are summarized in Figure 6.8.2 below (from Figure 2 in their paper). There has been a growing wildfire deficit over the 20th century. In other words, however much fire was observed in the 20th century, it was less than what would have been observed in previous centuries based on the climatic conditions. Parks *et al.* (2025) likewise find that despite the recent increase in wildfire burn area in North America, a significant wildfire deficit remains relative to historical wildfire regimes.

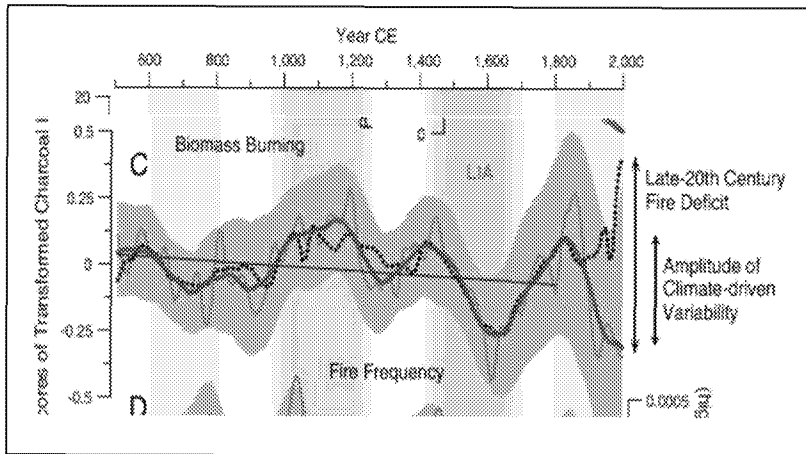


Figure 6.8.2 Fire frequency and fire deficit in the US. The red line shows the smoothed charcoal record and the black dotted line shows the predicted charcoal record based on climatic conditions model. Source: Marlon *et al.* (2012) Figure 2.

U.S. data from the National Interagency Fire Centre (NIFC) from 1926 to 2023 are shown in Figure 6.8.3. The NIFC has removed the pre-1960 data from its current website on the grounds that measurement methods changed after 1960 making the comparison unreliable. Nonetheless just focusing on the post-1985 interval the number of fires is not increasing. The area burned did increase but only until about 2007.

Forest fires have always been a part of nature, and they can certainly create conditions that are inhospitable in the short term for all life, including humans. But science has confirmed the overall benefit and necessity of forest fires. While recent high-profile fires and seasons serve as a reminder of the potential destructive impact, the highest profile U.S. forest fire remains the 1910 Big Blowup fire in the U.S. West, which destroyed over three million acres and eliminated entire towns like Taft, MT (Apple, 2020). The 1910 fire reshaped the U.S. Forest Service (National Forest Foundation 2022) leading to a focus on fire suppression with a primary goal of defeating all forest fires (Forest History Society, 2022). This led to the “10 am rule” in 1935 requiring that all fires spotted on any day had to be controlled by 10 am the following day (National Forest Foundation, 2022).

While defeating all fires seemed a noble goal, questions began to arise as to whether this behavior “followed the science” (U.S. Forest Service, 2022). Over time the U.S. Forest Service has begun to rethink its goals, recognizing that new approaches such as prescribed burns, fuel elimination, and controlled wildfires are more appropriate (Sommer, 2016). Recent research is validating this approach and recognizing that more frequent smaller fires likely result in healthier forests, water ecosystems and biodiversity (Stephens *et al.*, 2021).

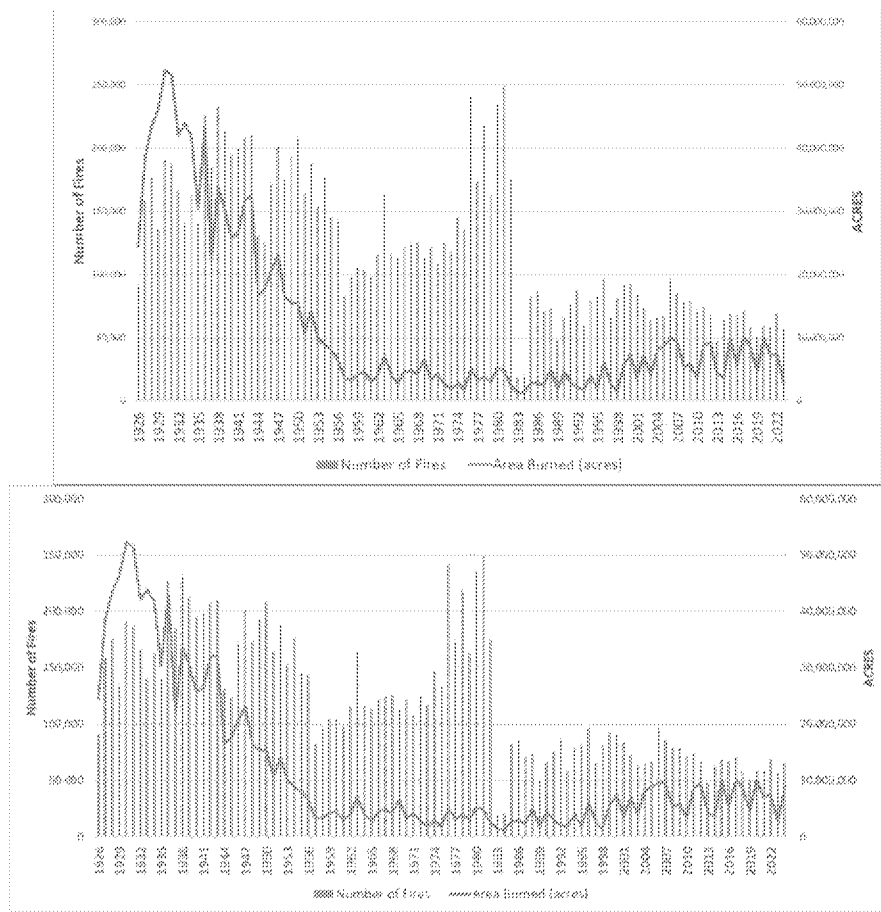


Figure 6.8.3: U.S. wildfires 1926 to 2023. Source: Post-2018: National InterAgency Fire Center data [HYPERLINK "<https://www.nifc.gov/fire-information/statistics/wildfires>"] Pre-2017 webarchive.org (n.d.).

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7 CHANGES IN SEA LEVEL

Chapter Summary

Since 1900, global average sea level has risen by about 8 inches since 1900. Sea level change along U.S. coasts is highly variable, associated with local variations in processes that contribute to sinking and also with ocean circulation patterns. The largest sea level rise along U.S. coasts are Galveston, New Orleans, and the Chesapeake Bay regions - each of these locations is associated with substantial local land sinking (subsidence) unrelated to climate change.

Extreme projections of global sea level rise are associated with an implausible extreme emissions scenario and inclusion of poorly understood processes associated with hypothetical ice sheet instabilities. In evaluating the IPCC AR6 projections to 2050 (with reference to the baseline period 1995-2014), almost half of the sea level rise has already occurred period has elapsed by 2025 with the sea level rising and at a lower rate than predicted. U.S. tide gauge measurements shown herein reveal no obvious acceleration beyond the historical average rate of sea level rise.

7.1 Global sea level rise

Global sea level rise is arguably the most important climate impact driver that is unambiguously associated with increasing temperatures. At the global level, warming raises sea level through thermal expansion of sea water and through melting of glaciers and ice sheets. Variations in land water storage are another important factor. At the regional scale, sea level change is influenced by large-scale ocean circulation patterns, and geologic processes and deformation from the redistribution of ice and water. Locally, vertical land motion from geologic processes, ground water withdrawal, and fossil fuel extraction are also important.

The IPCC AR6 estimates that global mean sea level increased by 7.9 (5.9–9.8) inches between 1901 and 2018, with the rate of sea level rise accelerating increasing in recent decades. At the ocean basin scale, sea levels have risen fastest in the Western Pacific and slowest in the Eastern Pacific over the period 1993–2018 (Fox-Kemper *et al.*, 2021). The rate of global sea level rise is estimated to be 0.12 inches/year (about the height of two stacked pennies) (NASA, 2020).

The observing systems for global sea level rise have advanced significantly in the satellite era, particularly with the advent of satellite altimeters in 1993. Local tide gauges have provided useful data for the past century, and even longer for a few locations. Following the end of the Little Ice Age in the mid-nineteenth century, tide gauges show that the global mean sea level began rising during the period 1820–1860, well before most anthropogenic greenhouse gas emissions.

7.2 U.S. sea level rise

Observed and predicted rates of mean global sea level rise might have little scientific relevance for specific locations, owing to local processes (NOAA, 2025). Figure 7.1 shows that in Canada and Alaska (and also northern Washington), sea level is decreasing, owing to uplift from glacial rebound. Most of the Pacific coast tide gauges show rises between 0.12 inches/year [missing words here], while largest U.S. rates are on the Gulf Coast (Louisiana and Texas) and in the mid-Atlantic states (Chesapeake Bay region).

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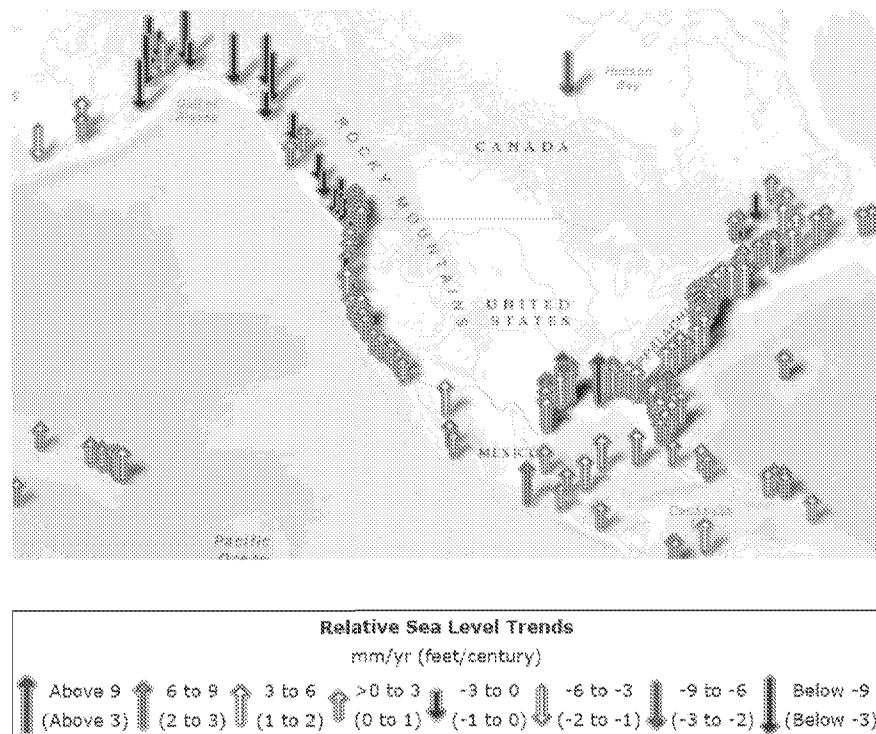


Figure 7.1. Map of rates of relative sea level rise along the U.S. coast (NOAA, <https://tidesandcurrents.noaa.gov/strends/>). For reference, 3 mm = 0.12 in.

Measurements of relative sea level rise from tide gauges conflate the climatologically relevant increase in the volume of seawater with local vertical land motion. The latter, which varies from place to place, is best measured using a Global Positioning System (GPS) station located near the tide gauge. It is driven by a range of processes that can be comparable to the climate signal and can locally exacerbate (subsidence/sinking) or mitigate (uplift) the risk of sea level rise (Wöppelmann and Marcos, 2016). Human activities triggering subsidence are often significant. They include soil drainage (e.g., for urban development purposes) and subsurface extraction of groundwater or hydrocarbons.

Table 7.1 shows absolute sea-level rise (ASLR) for selected locations, determined from the sum of uncorrected sea-level rise as estimated from tide gauge time series (Relative Sea Level Rise, RSLR) (NOAA, 2025) and the vertical land motion (VLM) measurements (NAS, 2012; Letetrel *et al.*, 2015; Karegar *et al.*, 2016). The absolute sea level rise for each of these locations is significantly smaller than the measured relative sea level rise owing to local subsidence. More than half of the measured relative sea level rise is attributed to land sinking for at these locations: San Francisco, Galveston, Grand Isle. For As noted above, reference, the global average rate of absolute sea level rise is estimated to be 0.12 inches/year.

Location	RSLR	VLM	ASLR
San Francisco, CA	+0.08	-0.06	+0.02
Galveston, TX	+0.26	-0.19	+0.07
Grand Isle, LA	+0.36	-0.28	+0.08
St Petersburg, FL	+0.12	-0.02	+0.10
New York City, NY	+0.11	-0.05	+0.06

Table 7.1 Absolute sea level rise (inches/year) consisting of Relative Sea Level Rise (RSLR) plus Vertical Land Motion (VLM)

San Francisco Bay

Over the past 100 years, relative sea level in the San Francisco Bay area has risen by 7.8 inches, at an average rate of 0.08 inches/year (Figure 7.2). As shown in Table 7.1, San Francisco's vertical land motion is -0.06 inches/year (sinking), producing a recent absolute rate of +0.02 inches/year. Portions of Treasure Island, San Francisco International Airport, and Foster City are sinking as fast as 0.4 inches/year (Shirzaei and Bürgmann, 2018). Problems in the San Francisco Bay area, including threats to the airport, are caused primarily by soil compaction in landfill zones that were formerly wetlands, not by the slow creep of global sea level rise.

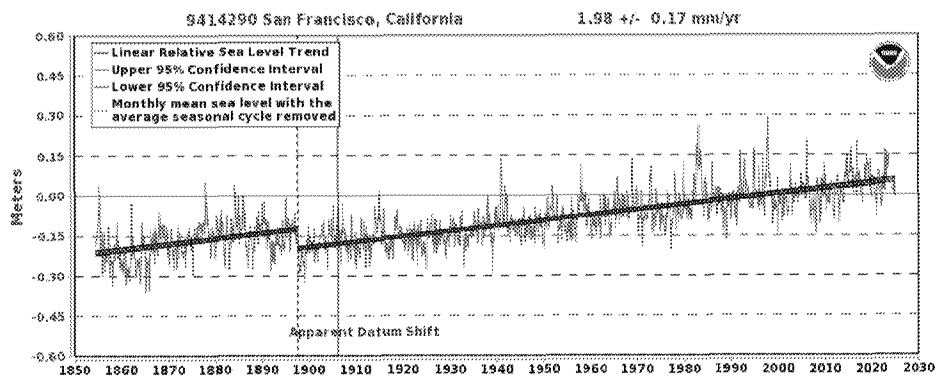


Figure 7.2. Tide gauge measurements at San Francisco, California, obtained from NOAA - https://tidesandcurrents.noaa.gov/strends/strends_station.shtml?id=9414290 (downloaded 4/22/25).

Galveston - Houston

Long-term tide measurements at Galveston Pier 21 show sea level rise of 2.18 feet in the past century, or a rate of 0.26 inches/year (Figure 7.3). Vertical land motion (subsidence) at Galveston is estimated at -0.19 inches/year, yielding an absolute rate of rise of +0.07 inches/year (Table 7.1). The U.S. Geologic Survey found that most of the land-surface subsidence in the Houston-Galveston region is a direct result of groundwater withdrawals (Kasmarek and Ramage 2017), which caused compaction of the aquifer sediments, mostly in the fine-grained silt and clay layers. By 1979, as much as 10 feet of subsidence had occurred in Houston.

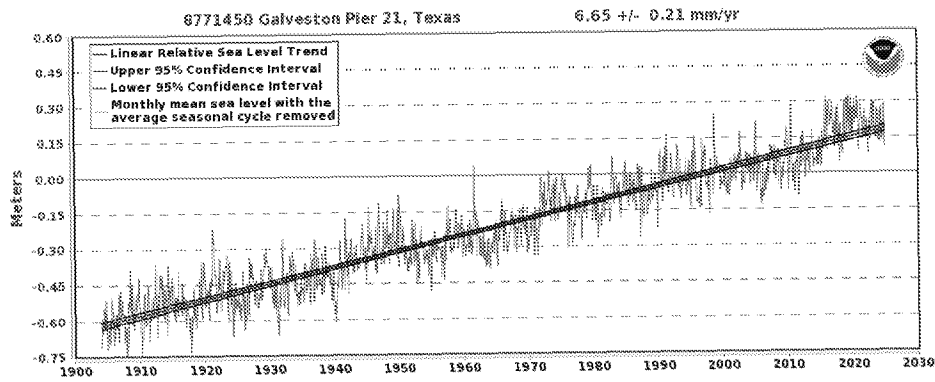


Figure 7.3. Tide gauge measurements Galveston Pier, TX, obtained from NOAA - https://tidesandcurrents.noaa.gov/sltrends/sltrends_station.shtml?id=8771450 (downloaded 4/22/2025).

New Orleans and the Mississippi delta

Long-term tide gauge measurements at Grand Isle, Louisiana, show that sea level has risen by slightly more than 3 feet over the last 100 years at an average rate of 0.36 inches/year (Figure 7.4). Vertical land motion (subsidence) is estimated -0.28 inches/year. Table 7.1 gives the absolute sea level rise as +0.08 inches/year.

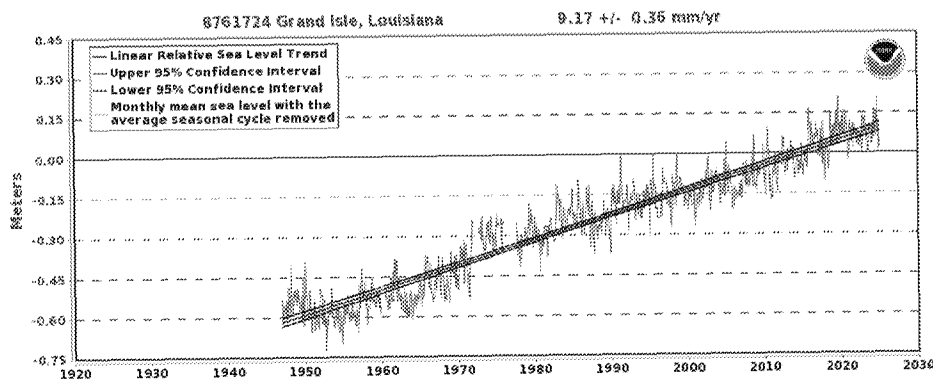


Figure 7.4. Tide gauge measurements at Grand Isle, LA, obtained from NOAA - https://tidesandcurrents.noaa.gov/sltrends/sltrends_station.shtml?id=8761724 (downloaded 4/22/25).

The issues of sea level rise and land loss in the Mississippi Delta region are complex, with geological subsidence and the decline in sediment transported by the Mississippi river being the dominant drivers. The construction of dams in the basin since the 1950s has decreased the Mississippi's suspended sediment load by ~50 percent (Maloney 2018). A new subsidence map of coastal Louisiana finds the coastal region to be sinking at about one third of an inch per year (Neinhuis *et al.* 2017), associated with groundwater and resource withdrawal. As the city's elevation averages one to two feet *below* sea level, sea level rise from anthropogenic warming is hardly the dominant driver of New Orleans' problems.

New York City

New York City is particularly vulnerable to the effects of sea level rise because it is built primarily on islands and has 520 miles of coastline. Measurements by a tide gauge at the southern tip of Manhattan (The Battery) show that relative sea level has risen ~~over~~ by more than 11 inches over the past century, at an average rate of 0.11 inches/year (Figure 7.5). But vertical land motion in the New York City area is -0.05 inches/year (roughly 5 inches per century), so that the absolute rate of sea level rise at The Battery is 0.06 inches/year, or about 55 percent of the measured relative sea level rise.

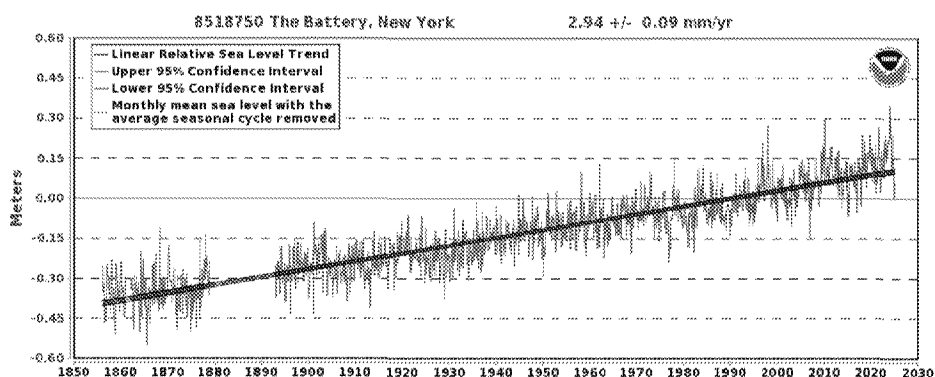


Figure 7.5. Tide gauge measurements at The Battery, New York, obtained from NOAA - https://tidesandcurrents.noaa.gov/sltrends/sltrends_station.shtml?id=8518750 (downloaded 4/22/25).

7.3 Projected sea level rise

The concern over sea level rise is not about the roughly 8 inches of global rise since 1900. Rather, it is about projections of *accelerated* rise based upon simulations of a warming climate through the 21st ~~Century~~century.

The IPCC AR6 finds *high agreement* across published global mean sea level projections for 2050 with little sensitivity to emissions scenarios. Considering only projections incorporating ice-sheet processes in whose quantification there is at least *medium confidence*, the global sea level projections for 2050, across all emissions scenarios, fall between 3.94 and 15.75 inches (5th–95th percentile *very likely* range) relative to the 1995–2014 baseline period (Fox-Kemper *et al.*, 2021).

Conversely, AR6 states there is *low agreement* across published global mean sea level projections for 2100, particularly for higher emissions scenarios. Considering only projections representing ice sheet processes in whose quantification there is at least *medium confidence*, the AR6 global mean sea level projections for 2100 lie between 7.87 and 39.37 inches (5th–95th percentile *very likely* range) under the medium emissions scenario SSP2–4.5 (Fox-Kemper *et al.*, 2021). There is deep uncertainty surrounding projections of sea level rise to 2100 owing to uncertainties in ice sheet instabilities, particularly for the higher emissions scenarios.

In evaluating the IPCC AR6 projections to 2050 (with reference to the baseline period 1995–2014), almost half of the sea level rise has already occurred. Time has already elapsed by 2025. From the tide gauge measurements in Figures 7.2–7.5, there is little to no obvious acceleration in sea level rise beyond the historical average rate. *[original wording is confusing – could be taken to mean have of the projection has already been realized]*

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In February 2022, NOAA issued its projections of sea level rise for various sites along the U.S. coast (Sweet *et al.*, 2022). They claim that by 2050, the sea will have risen one foot at The Battery in Manhattan (relative to 2020). A one-foot rise in thirty years would be more than twice the current rate and about three times the average rate over the past century. In that historical context, NOAA’s projection is remarkable—as shown in Figure 7.6, it would require a dramatic acceleration beyond anything observed since the early 20th century. But even more noteworthy is that they Sweet *et al.* (2022) say this rise is “locked in”—it will happen no matter what future emissions are. We should know in a decade or so whether that prediction has legs.

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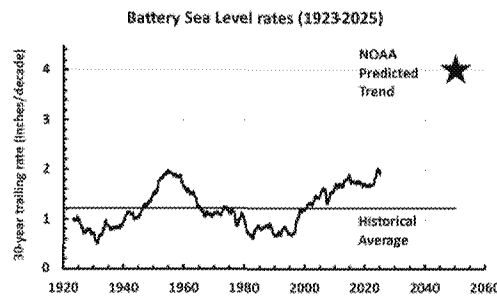


Figure 7.6 Rate of sea level rise at the Battery in Manhattan. Shown is the historical thirty-year trailing trend, together with the allegedly “locked in” NOAA predicted trend for 2050. Historical data: NOAA Tides and Current.

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8 UNCERTAINTIES IN CLIMATE CHANGE ATTRIBUTION

Chapter summary

“Attribution” refers to identifying the cause of some aspect of ~~climate change~~ the changing climate, specifically with reference to anthropogenic activity. There is an ongoing scientific debate around attribution methods, particularly regarding extreme weather events. Attribution is made difficult by high natural variability, the relatively small expected anthropogenic signal, lack of high-quality data, and reliance on deficient climate models. The IPCC has long cautioned that methods to establish causality in climate science are inherently uncertain and ultimately depend on expert judgement.

Substantive criticism of the main IPCC assessments of the role of CO₂ in recent warming focus on inadequate assessment of natural climate variability, uncertainties in measurement of solar variability and in aerosol forcing, and problems in the statistical methods used for attribution.

The IPCC does not in general make attribution claims for most climate impact drivers related to extreme events. Statements related to statistics of global extremes (e.g. event probability or return times, magnitude and frequency) are not generally considered accurate owing to data limitations and are made with low confidence. Attribution of individual extreme weather events is challenging due to their rarity. Conflicting claims about the causes of the 2021 Western North America Heatwave illustrate the perils of hasty attribution claims about individual extreme events.

8.1 Introduction

The Intergovernmental Panel on Climate Change (IPCC) distinguishes between detection of a climate change and attribution of its cause. As defined by the IPCC AR6 WGI Glossary (IPCC, 2025):

Detection: Detection of change is defined as the process of demonstrating that climate or a system affected by climate has changed in some defined statistical sense, without providing a reason for that change. An identified change is detected in observations if its likelihood of occurrence by chance due to internal variability alone is determined to be small, for example, <10%.

Attribution: Attribution is defined as the process of evaluating the relative contributions of multiple causal factors to a change or event with a formal assessment of confidence.

Both ~~steps~~ detection and attribution rely on statistical analysis. Detection focuses on whether changes are significant enough to stand out from random variation regardless of cause. Attribution involves comparison of observed events to model-generated counterfactuals. Since experimentation on the climate is not possible, attribution requires statistical inference to assess how much human activities, such as GHG emissions, versus natural factors, like volcanic eruptions, contribute to observed changes. Attribution methods assume all external and internal drivers of the system are known and represented.

There are ongoing scientific debates around attribution methods, especially those for attributing extreme weather events to climate change. The IPCC has long cautioned that methods to establish causality in climate science are inherently uncertain and ultimately depend on expert judgement. AR4 Working Group I (Hegerl *et al.*, 2007) explained it as follows:

‘Attribution’ of causes of climate change is the process of establishing the most likely causes for the detected change with some defined level of confidence... unequivocal attribution would require controlled experimentation with the climate system. Since that is not possible, in practice attribution of anthropogenic climate change is understood to mean demonstration that a detected change is ‘consistent with the estimated responses to the given combination of anthropogenic and natural forcing’ and ‘not consistent with alternative, physically plausible explanations of recent climate change that exclude important elements of the given combination of forcings’ (IPCC, 2001)... The approaches used in detection and attribution research described above cannot fully account for all uncertainties, and thus ultimately expert judgement is required to give a calibrated assessment of whether a specific cause is responsible for a given climate change.

AR5 Working Group II (Cramer *et al.*, 2014) makes the following statement:

Two broad challenges to the detection and attribution of climate change impacts relate to observations and process understanding. On the observational side, high-quality, long-term data relating to natural and human systems and the multiple factors affecting them are rare. In addition, the detection and attribution of climate change impacts requires an understanding of the processes by which climate change, in conjunction with other factors, may affect the system in question.

Because of the complexity of the causal chains in the climate system, investigation of these relationships is exceptionally challenging.

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8.2 Attribution methods

The IPCC employs several attribution methods to assess the causes of observed climate changes, distinguishing between natural and human-induced factors. Below is a concise description of the key IPCC attribution methods.

Optimal Fingerprinting uses linear regression to explain variations in observed climate data as a weighted sum of climate model simulations run with and without anthropogenic forcings. The data used in the regression model is weighted to try to minimize the influence of random noise and the estimation method is chosen to account for climate model error.

Time Series Analysis exploits statistical differences between anthropogenic forcing and natural variability to see which dominates observed temperatures and also uses variations in the timing of changes to determine if causal dependence across data series can be inferred.

Process-Based Attribution focuses on understanding the physical mechanisms behind observed changes. This approach combines observations, climate models, and theoretical understanding to attribute changes in specific processes to forcings. This approach is often used for regional climate phenomena, such as monsoon changes or polar amplification

Extreme Event Attribution assesses the role of human influence in the likelihood of occurrence or intensity of extreme weather events (e.g. heat waves or droughts). Methods include:

- Probabilistic Event Attribution uses large ensembles of climate model simulations to compare observed events to model-generated counterfactuals
- Storyline Approach examines the physical processes driving an event and evaluates how anthropogenic forcings might have modified those processes

8.3 Attribution of global warming

Attribution statements for global warming in the three most recent IPCC Assessment reports are **[boldface emphasis added]**:

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AR4: **Most of the observed increase** in global average temperatures since the mid-20th century is *very likely* due to the observed increase in anthropogenic greenhouse gas concentrations. (IPCC 2007)

AR5: It is *extremely likely* that **more than half of the observed increase** in global average surface temperature from 1951 to 2010 was caused by the anthropogenic increase in greenhouse gas concentrations and other anthropogenic forcings together. The best estimate of the human-induced contribution to warming is similar to the observed warming over this period. (IPCC 2013)

AR6: The *likely* range of total human-caused global surface temperature increase from 1850–1900 to 2010–2019 is 0.8°C to 1.3°C, with a best estimate of 1.07°C. It is *likely* that well-mixed GHGs contributed a warming of 1.0°C to 2.0°C, other human drivers (principally aerosols) contributed a cooling of 0.0°C to 0.8°C, natural drivers changed global surface temperature by –0.1°C to +0.1°C, and internal variability changed it by –0.2°C to +0.2°C. It is *very likely* that **well-mixed GHGs were the main driver** of tropospheric warming since 1979. (IPCC 2021)

The AR4 and AR5 attribution statements reference the warming since the mid-20th century, the period when greenhouse gas emissions began rapidly increasing. The words “most” and “more than half” are deliberately imprecise, potentially ranging from 51 to 99 percent of the warming – presumably this imprecision is to account for structural uncertainties such as natural internal variability. The confidence level increases from *very likely* to *extremely likely* from AR4 to AR5. The structure of the attribution statement in the AR6 is fundamentally different, referencing the warming to the period 1850–1900. The AR6 attribution statement is more precise numerically, but with a lower level of confidence at “likely” – the AR6 attributes essentially all the warming to increases in greenhouse gases. The most confident statement from the AR6 relates to the tropospheric warming since 1979, using the words “main driver” and “*very likely*.”

There are three areas of substantive criticism of the IPCC’s assessment of the causes of the recent warming: inadequate assessment of natural climate variability, inappropriate statistical methods, and substantial discrepancies between models and observations. The last is discussed in Chapter 5, while this chapter discusses the first two factors. All of these criticisms are relevant to the IPCC’s policy-informing attribution of the recent warming, which also underpins extreme event attribution

8.3.1 Natural climate variability

IPCC AR6 states that natural external drivers since 1850–1900 have changed global surface temperature by –0.1°C to +0.1°C, and internal variability has changed it by –0.2°C to +0.2°C – on average having essentially no net impact on the warming since 1850–1900. As discussed below, this minimal contribution of natural variability has been disputed by several publications that question the magnitudes of solar variability and internal variability from large-scale ocean circulation.

Solar variability

IPCC AR5 concluded that the best estimate of radiative forcing due to Total Solar Irradiance (TSI) changes over the period 1750–2011 was very small (0.05 W/m², Myrhe *et al.*, 2014). IPCC AR6 acknowledges substantially higher values and a much larger range of estimates of changes in TSI over the

last several centuries, stating that the TSI between the Maunder Minimum (1645–1715) and the second half of the twentieth century increased by 0.7–2.7 W/m², a range that includes both low and high variability TSI data sets (Gulev, 2021). However, the recommended forcing dataset for the CMIP6 climate model simulations used in AR6 for attribution studies averages two data sets with low solar variability (Matthes, 2017).

While AR6 shows a substantially greater solar impact than does AR5, the overall impact of solar forcing on the climate was still assessed to be small compared to anthropogenic forcing. However, the impact of solar variations on the climate is uncertain and subject to substantial debate (Lockwood, 2012; Connolly *et al.*, 2021) - something that is not evident from the IPCC assessment reports (Lockwood, 2012; Connolly *et al.*, 2021).

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The change variations of TSI over time remains remain a challenging problem. Since 1978, there have been direct measurements of TSI from satellites. However, the data exhibits non-negligible inconsistencies, and interpreting any multi-decadal trends in TSI requires comparisons of observations from overlapping satellites. There are several rival composite TSI datasets, disagreeing as to whether TSI increased or decreased during the period 1986–96 (the ACRIM gap; see Chapter 4). Further, the satellite record of TSI is used to calibrate proxy models, whereby past solar variations are inferred from sunspots and cosmogenic isotope measurements (Velasco Herrera *et al.*, 2015).

There is substantial evidence for high solar activity in the second half of the 20th century (starting in 1959) and extending into the 1990's, before a decline in the early 21st century; this period is often termed the “Modern Maximum.” (Chatzistergos *et al.*, 2023; Solanki *et al.*, 2004; Usoskin *et al.*, 2007). However, some scientists have concluded that it is not possible to be confident of any multi-decadal trend in TSI (Schmutz, 2021).

This uncertainty causes some reconstructions of TSI from 1750 to have low variability (implying a very low impact of solar variations on global mean surface temperature) whereas datasets with high TSI variability can explain more than 70 percent of the temperature variability since preindustrial times (Scafetta, 2013; Stefani, 2021). The choice of TSI satellite record used in an analysis can therefore substantially influence how much climate change is attributed to human versus natural forcings.

There is growing evidence that other aspects of solar variability, which are referred to as solar indirect effects, either amplify TSI forcing or are independent of TSI forcing. Scafetta *et al.* (2023) suggests that ~80 percent of solar influence on climate may stem from non-TSI mechanisms. There are numerous candidate processes, including solar ultraviolet changes; energetic particle precipitation; atmospheric-electric-field effect on cloud cover; cloud changes produced by solar-modulated galactic cosmic rays; large relative changes in the magnetic field; and the strength of the solar wind. Such solar indirect effects are not included in climate models, although indirect methods of estimating their impacts suggest they are significant. However, claims of non-TSI mechanisms influencing climate are uncertain and debated.

Natural variability of large-scale ocean circulations

Variations in global mean surface temperature are linked to recurrent large-scale variations in ocean circulation patterns, including the Atlantic Multidecadal Oscillation (AMO), the Pacific Decadal Oscillation (PDO) and the El Niño-Southern Oscillation (ENSO). These circulations influence ocean heat uptake and heat distribution and also influence atmospheric circulation patterns and cloud distributions. There is some debate as to whether these variations are strictly internal to the climate system, or whether this variability can have a solar/astronomical origin or can be influenced by large volcanic eruptions.

While climate models simulate the large-scale ocean circulations and internal climate variability, most models show have too little amplitude compared to observations at multi-decadal frequencies and phasing out-of-sync with the observed climate (Kravtsov *et al.* 2024). Averaging multiple simulations effectively

averages out the internal variations, leaving only the forced climate variability (*e.g.* CO₂ forcing). With most of the modern warming beginning in the late 1970's, the recent 50-year warming is on the same time scale as the multi-decadal oscillations of the AMO and PDO.

Here are summary statements from the IPCC AR5 and AR6 reports:

AR5: Decadal variability in the Pacific, associated with the PDO or IPO [Interdecadal Pacific Oscillation], contributes significantly to regional and global temperature trends, but the relative contributions of internal variability and external forcing are difficult to disentangle in CMIP5 simulations.

AR6: Since AR5, there has been increased understanding of the role of internal variability, such as ENSO, PDO, and AMO, in modulating regional climate trends. However, limitations in simulating the exact timing and amplitude of these modes in CMIP6 models contribute to uncertainties in attributing observed changes to anthropogenic forcing (*high confidence*).

AR6 assessed the amplitude of the peak-to-trough impact of the multi-decadal oscillations on global temperatures has been assessed by the IPCC AR6: "The likely range of change due to internal variability is -0.2°C to +0.2°C (IPCC, 2021)." This implies a trough-to-peak change of 0.4°C. Over many centuries, any global temperature changes from the troughs and peaks will cancel out, with little to no net impact.

However, with a nominal timescale of 60-80 years for oscillations such as the AMO and PDO, the timing of the peaks and troughs can be confused with the secular trend. This becomes very relevant for attributing the warming for the past 50 years, when most of the recent warming has occurred. Even if climate models have the correct amplitude of the multidecadal oscillations, the timing of the peaks and troughs is not adequately simulated, with each model and individual ensemble simulating a different phasing. Once the simulations of the ensemble members and different models are averaged, the natural internal variability contribution is averaged out owing to differences in phasing, effectively cancelling the role of natural internal variability in the attribution process.

The time series of global surface temperature anomalies since 1850 (Figure 8.1) shows irregular variations of significant amplitude against the background of an overall warming trend and, after 1976, a pronounced difference in trends between the Northern Hemisphere and the Southern Hemisphere. The period between 1905-1945 shows a strong trend of warming. The following ~~thirty~~ 30 years from 1945-1976 showed slightly declining temperatures. The most recent warming period started in 1977.

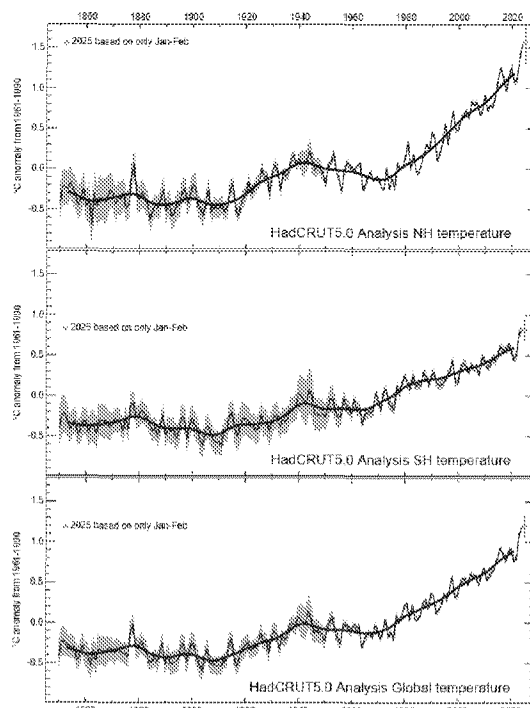


Figure 8.1 Global average surface temperature anomalies 1850—2025. Top: Northern hemisphere. Middle: Southern hemisphere. Bottom: Global average. Source: UK Hadley Centre [[HYPERLINK "https://crudata.uea.ac.uk/cru/data/temperature/HadCRUT5.0Analysis.pdf"](https://crudata.uea.ac.uk/cru/data/temperature/HadCRUT5.0Analysis.pdf)]

The causes of the early 20th century warming are discussed by Hegerl *et al.* (2017; 2019). The atmospheric CO₂ concentration increased from 298 ppm in 1905 to 310 ppm in 1941, implying that CO₂ had little impact. Volcanic activity during this period was very low, and solar forcing is uncertain. Yet Hegerl *et al.* (2017) somehow inferred that 40-54 percent of this warming could be attributed to external forcing, with the rest associated with internal variability. Bronniman *et al.* (2024) focused on the causes of the cooling in the first decade of the 20th century in the Southern Hemisphere. They found that the cooling was related to a La-Niña-like pattern in the Pacific, a cold tropical and subtropical South Atlantic, a cold extratropical South Pacific, and cool southern midlatitude land areas. The Southern Annular Mode was positive, with a strengthened Amundsen–Bellingshausen seas low, although the spread of the data products is considerable.

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The warming in the 1930's and subsequent cooling during mid-century was particularly pronounced in the Arctic. Bokuchava and Semenov (2021) find that these variations were most likely caused by a combined effect of long-term natural climate variations in the North Atlantic and North Pacific with a contribution of the natural radiative forcing related to the reduced volcanic activity and variations of solar activity, as well as growing greenhouse gases concentrations. Tokinaga *et al.* (2017) showed that the combined effect of internally generated Pacific and Atlantic interdecadal variability intensified Arctic

warming in the early 20th century. The synchronized Pacific-Atlantic warming drastically alters planetary-scale circulations over the Northern Hemisphere; these same circulation patterns have a global influence.

The cooling period between 1945 and 1976 has been referred to as the “grand hiatus.” Numerous causes have been hypothesized: natural internal variability from fluctuations associated with the Pacific Decadal Oscillation and Atlantic Multidecadal Oscillation; cooling from increased emissions of aerosols from industrial activities; increased heat uptake in the Atlantic, Southern and Equatorial Pacific Oceans. Thompson *et al.* (2010) find that the hemispheric differences in temperature trends in the middle of the twentieth century stem largely from a rapid drop in Northern Hemisphere sea surface temperatures of about 0.3 °C between about 1968 and 1972.

The Great Pacific Climate Shift of 1976-1977 was a notable climatic event characterized by an abrupt change in the North Pacific Ocean's atmosphere-ocean system that interacted with global climate patterns. This shift is closely associated with the Pacific Decadal Oscillation (PDO) that oscillates between warm and cool phases over decades. The 1976 shift marked a transition from a predominantly negative (cool) PDO phase (1947–1976) to a positive (warm) phase (1977 through 2000), with significant implications for global and regional climate patterns including the frequencies of El Niño and La Niña events. The Great Pacific Climate Shift coincided with the beginning of a period of accelerated global warming. When the Great Pacific Climate Shift is accounted for in climate attribution analyses since 1950, 40 percent or more of the warming in the second half of the 20th century is attributed to natural internal variability (McLean *et al.*, 2009; Tung and Zhou, 2013; Chylek *et al.*, 2016; Scafetta, 2021).

8.3.2 Optimal fingerprinting

Optimal fingerprinting is a statistical technique introduced by Allen and Tett (1999) that compares observed climate data to climate model simulations to identify patterns (or “fingerprints”) associated with human or natural forcing. It involves taking a vector of observed climate changes, such as warming rates in locations around the world, and decomposing them into a weighted sum of “signals”, which are analogues to the observations generated/simulated by climate models with/under different types of forcings. The weights are chosen using a regression method called Total Least Squares. The analysis commonly uses just two signals, one generated by models using only anthropogenic forcings and one using only natural forcings. If the estimated weighting coefficient attached to a signal is significantly different from zero, then that signal is said to be “detected”. If it is close to 1.0, then the model generating the signal is said to be consistent with observations. If it is less than 1.0, then the model signal for that forcing is too strong and needs to be scaled down, and vice-versa. Both the observed data and the signals are weighted using a model-generated estimate of patterns of randomness in the climate system so as, in principle, to put maximum weight on regions where natural variability is minimized.

Optimal fingerprinting is the primary tool for attribution in the research literature. While it has been widely used and prominently featured by the IPCC since 2001, there is very little literature examining the statistical properties of the results it generates. One of its inherent weaknesses is that results depend on assumptions about the accuracy of climate models, especially regarding their representation of natural variability (IPCC AR5 Ch 10.2.3).

A series of papers by McKittrick (McKittrick 2021, 2022, 2023, 2025) has challenged the optimal fingerprinting method, arguing that it is inherently unreliable and that practices adopted from econometrics can provide more valid results. Statistical theory requires that for regression models to yield unbiased coefficients, a set of assumptions called the Gauss-Markov conditions must hold. McKittrick (2021) argued that the Allen and Tett (1999) methodology violates the Gauss-Markov conditions, leading to potentially biased and inconsistent fingerprinting coefficients, undermining the reliability of the method. Chen *et al.* (2023) confirmed this analysis and although they argued that the method *could* yield unbiased valid results, but only under very stringent assumptions. McKittrick (2022) and (2023) raised a further concern that climate scientists—

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virtually alone among scientific disciplines—have used an estimation method called Total Least Squares (TLS) to estimate anthropogenic greenhouse gas signal coefficients, despite its tendency to be unreliable unstable unless some strong assumptions hold that in practice are unlikely to be true in practice. Under conditions that easily arise in optimal fingerprinting, TLS estimates have a large positive bias. Thus, any study that used TLS for optimal fingerprinting without verifying its applicability in the specific data context has likely overstated the attribution.

McKittrick (2025) presented an empirical example comparing the results of conventional optimal fingerprinting against methods drawn from mainstream econometrics that are known to be valid for the specific application of signal detection. While the IPCC optimal fingerprinting method yields an anthropogenic signal coefficient close to 1.0 on a global temperature data set spanning 1900 to 2010, consistent estimation yields a coefficient around 0.4, which rises to about 0.65 ~~on for~~ data spanning 1980 to 2010, implying the model response to greenhouse gases needs to be scaled down by about half to optimally match observations. The natural forcing NEED TO SAY WHAT THIS IS – VOLCANIC, T.S.I. ?? signal coefficient, by contrast, is between 2.0 and 4.0, implying the climate model signals of natural forcing need to be scaled up two-to four-fold to match observed climate change. The fingerprinting coefficients estimated in McKittrick (2025), when used to scale the average sensitivity of the climate models used to generate the forcing signals in his data set, imply a Transient Climate Sensitivity of 1.4°C, which is consistent with the estimate by Lewis (2023) using a different estimation method and multiple independent data sets.

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While these analyses do not falsify all results stemming from the IPCC optimal fingerprinting method, they do challenge the basis on which they were believed to be correct.

8.3.3 Time series methods

The IPCC (AR5 WGI 10.2.2) drew attention to alternative approaches ~~of to~~ assessing causality that emerged from the time series econometrics literature. These have the advantage of not depending on assumptions about the accuracy of climate models, but the disadvantage that they depend on difficult-to-test assumptions about the data generating process underlying climate and forcing data. The methods, now referred to as climate econometrics, use the tools of unit root testing, Granger causality and cointegration analysis, all of which are familiar in economics and finance and which are slowly being adopted in climate science. Time series analysis methods hold out the possibility of determining whether anthropogenic or natural forcings are the primary drivers of climate change without requiring the use of climate models, although key questions remain unsettled (e.g. Dergiades *et al.*, 2016, Balcombe *et al.*, 2019, Dagsvik *et al.*, 2020, Razzak, 2022, Dagsvik and Moen, 2023).

“Granger causality” modeling is a tool for determining directions of influence in data series that move together. It is a statistical, not a physical, concept. If knowing the current value of one variable significantly improves the forecast accuracy of another variable, we can infer a causal connection exists; this is called Granger causality. The modeling tools, called vector autoregression, measure direct impulse-response patterns and feedbacks. An application to the well-known Vostok ice core data revealed an error in Al Gore’s documentary *An Inconvenient Truth*. Gore showed the data and drew attention to the coherence of temperature and CO₂ changes over a 440,000 year span, which he asserted was due to CO₂ driving temperature changes. But temperature changes can also affect atmospheric CO₂ levels. Davidson *et al.* (2015) examined the series and found that temperature Granger causes CO₂ but not the reverse. In other words on the time scales represented in the Vostok data the coherence in the series is primarily due to the influence of temperature on CO₂ levels, not the feedback of CO₂ levels on temperature.

In summary, the primary statistical methods for attributing causation in climate data are optimal fingerprinting and time series analysis. Applications of the Allen and Tett (1999) optimal fingerprinting

method dominate the attribution literature and have underpinned past IPCC conclusions, but results depend on the accuracy of climate models and the method has recently been criticized as inherently biased. Time series methods do not depend on climate models but require assumptions of their own and have generated results that have thus far not converged on a consensus.

8.4 Declining planetary albedo and recent record warmth

A sharp recent increase in global average temperatures has raised the question of short-term drivers of climate. One such candidate is the fraction of absorbed solar radiation, which has also increased abruptly in recent years. The question to be answered is whether the change is an internal feedback to warming caused by greenhouse gases, or whether something else increased the fraction of absorbed radiation, which then caused the recent warming.

The albedo is the fraction of incoming solar radiation that is reflected back into space rather than being absorbed by the planet. Highly reflective surfaces like cloud tops and snow and ice are most important in this regard. The Earth's albedo is approximately 30 percent, meaning almost a third of the sunlight that reaches Earth is directly reflected back to space. A lower albedo implies more solar energy is absorbed by the planet to be then re-radiated as heat. Hence, other things being equal, a decline in albedo is associated with a warming of the Earth.

Arguably the most striking change in the Earth's climate system during the 21st century is a significant reduction in planetary albedo since 2015, which has coincided with at least two years of record global warmth. Figure 8.2 shows the planetary albedo variations since 2000, when there are good satellite observations. The 0.5 percent reduction in planetary albedo since 2015 corresponds to an increase of 1.7 W/m² in absorbed solar energy averaged over the planet (Hansen and Karecha, 2025). For comparison, the IPCC estimate of forcing from the increase in atmospheric CO₂ compared to preindustrial times is approximately 2.2 W/m² (Figure 3.1.2).

Looking back prior to 2000 with less adequate data, Goessling *et al.* (2025) assessed that planetary albedo was relatively low around the 1940's and 50's, before rising industrial aerosol precursor emissions increased albedo until the 1980's. The strongest planetary albedo excursions were high-albedo episodes caused by volcanic eruptions, such as after the Mount Pinatubo eruption in 1991. Although uncertain, the 2023 planetary albedo minimum may have been the lowest since at least 1940.

Changes in surface characteristics can't explain this decrease in planetary albedo since 2015:

- Arctic sea ice extent has declined by about 5 percent since 1980 ([HYPERLINK "https://nsidc.org/data/seaice_index/images/s_plot_hires.png"]), although following 2007 there has been a pause in the Arctic sea ice decline (England *et al.*, 2025)
- Regarding Antarctic sea ice, the IPCC AR6 concludes that "There has been no significant trend in Antarctic sea ice area from 1979 to 2020 due to regionally opposing trends and large internal variability." (Summary for Policymakers, A.1.5)
- Northern hemispheric annual snow cover has been slowly declining since 1967, with barely significant trends. The data show the Northern Hemisphere has snowier winters, accompanied by more rapid melt in spring and summer. [HYPERLINK "<http://climate.rutgers.edu/snowcover/>"]
- Global greening (Chapter 2) is contributing to the decrease in planetary albedo, as forests have a lower albedo than open lands or snow. However, there is some evidence that forests increase cloud cover, whose (high reflectivity), which counteracts the direct albedo increase associated with increasing forested area.

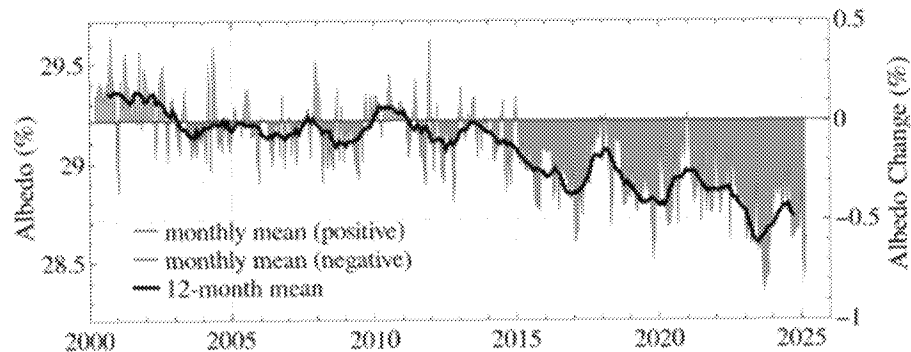


Figure 8.2. Earth's albedo (reflectivity, in percent), with seasonality removed. From Hansen and Karecha (2025)

Changes in surface reflectivity are contributing to a small, slow decline in planetary albedo. However, these changes cannot explain the sharp decline in planetary albedo beginning in 2015. Further, any changes in surface albedo are effectively damped by about a factor 3 on average, primarily due to cloud masking (Loeb *et al.* 2019). Surface albedo changes have thus contributed only weakly to the recent planetary albedo decline, particularly when averaged annually and globally. This leaves the most likely explanation for the sharp albedo decline as changes in atmospheric aerosols and/or clouds.

Aerosols are small particles in the atmosphere that reflect sunlight and can interact with cloud processes to make the clouds more reflective. Aerosols have both natural origins (e.g. dust from soils, wildfires) as well as human origins from combustion (including fossil fuels). Regions that are greening and/or have an increase in rainfall might be producing less dust. For anthropogenic aerosols, new ship fuel regulations aimed at reducing sulfur emissions were implemented in three phases, in 2010, 2015 and 2020. Sulfate particles reflect solar radiation. Cleaner air means less solar radiation is reflected, which contributes to surface warming. Indirect effects of sulfate aerosols include increasing the reflectivity of low-level clouds in the subtropics. There is controversy surrounding the importance of this change in sulfate emissions to the Earth's radiation balance (Hodnebrog *et al.*, 2024; Yuan *et al.*, 2024), but the restriction of this effect to major shipping routes suggests a small global impact (Schmidt, 2024).

Changes in clouds are the primary candidate to explain the decline in global albedo since 2015. Two recent papers (Loeb *et al.*, 2024; Goessling *et al.* 2024) have addressed recent variations in cloud properties. By considering satellite and reanalysis data, Loeb *et al.* found that decreases in low- and mid-level clouds since 2015 are the primary reason for decreasing planetary albedo in the Northern hemisphere, whereas in the Southern hemisphere the decrease in planetary albedo is primarily due to decreases in mid-level clouds across all latitude zones. Goessling *et al.* (2024) found that cloud anomalies were mainly due to reduced low-level clouds. Regions with coherent low-level cloud reductions over the past decade include the warm pool region around the Maritime Continent and the northern extratropical western Pacific, as well as large parts of the Atlantic and adjacent land regions. The reduction of global cloud cover identified in these analyses since 2015 is 1-2 percent.

The question then becomes "What caused the change in cloud cover?": Two causes have been posited for the declining cloud cover over the past decade:

- Natural climate variability

- Changes in low cloud cover associated with warming sea surface temperatures, implying an emerging positive feedback to climate change (Hansen and Karecha, 2025)

It is not easy to justify the existence of a new positive low cloud feedback that began emerging in 2015 is not easy to justify. However, there are numerous natural climate signals during this period that are associated with atmospheric circulation changes that can influence the distribution of clouds:

- The 2014-2016 was one of the strongest El Niño events on record
- A cold anomaly beginning in 2015 in the subpolar gyre of the North Atlantic reflects a shift in the ocean circulation pattern associated with decadal variability in the Atlantic (Frajka-Williams *et al.*, 2017; Arthun *et al.* 2021).
- The Pacific Decadal Oscillation positive index peaked in 2016, then declined and has been in negative territory since late 2019
- Eruption of the submarine Hunga-Tonga volcano in 2022

Interannual cloud anomalies associated with the El Niño Southern Oscillation (ENSO) have a significant global signal and strong regional signals especially over the tropical Indian and Pacific Oceans.

The Hunga Tonga eruption is remarkable in its coincidence with the exceptionally low value of planetary albedo in 2023. Goessling *et al.* (2024) found that the cloud perturbations in 2023 showed a different pattern from the overall pattern since 2015, with reduced cloud cover being most pronounced in the northern hemisphere and tropics. Regional reductions in cloud cover were most pronounced over the eastern Indian Ocean, over South America and extending over the eastern Pacific, as well as over northern North America, in the Southern Ocean around 60S, in the subtropical and eastern North Atlantic, and in parts of the North Pacific.

The Hunga Tonga eruption was unusual in that it injected large amounts of water vapor into the stratosphere, in addition to sulfate particles. Early publications focused on the impacts to stratospheric circulations and the direct radiative impact. In 2025, papers are emerging that examine the indirect effects of Hunga Tonga on surface climate using ensemble simulations from earth system models (Bednarz *et al.*, 2025; Kuchar *et al.*, 2025). These papers found statistically significant impacts on regional climates that were driven by circulation changes from couplings between the stratosphere and troposphere. These papers indicate the complex interactions between volcanic activity and climate dynamics. More research is needed to untangle any impacts of Hunga Tonga eruption on planetary albedo.

In summary, the decline in planetary albedo and the concurrent decline in cloudiness have emphasized the importance of clouds and their variations to global climate variability and change. A change of 1.2 to 2-percent in global cloud cover has a greater radiative impact on the climate than the direct radiative effect of doubling CO₂. While it is difficult to untangle causes of the recent trend, the competing explanations for the cause of the declining cloud cover have substantial implications for assessing the Equilibrium Climate Sensitivity and also for the attribution of the recent warming. An additional 10 years of data should help clarify whether this is a strong positive cloud feedback associated with warming, or a temporary fluctuation driven by natural variability

8.5 Attribution of climate impact drivers

The IPCC (Ranasinghe *et al.* 2021) defines “climate impact drivers” or CIDs as “physical climate system conditions (e.g., means, events, extremes) that affect an element of society or ecosystems.” Hence CIDs are those features of the weather and climate system of primary interest in assessing the impacts of climate change since they potentially affect humans and the natural world. For instance, under the heading “Heat and Cold,” CIDs are identified as mean air temperature, extreme heat, cold spells and frost. The IPCC

also points out that CIDs are not necessarily harm-related: depending on the system in question they can be detrimental, neutral, beneficial or a combination.

The first column of Table 12.12 in Ranasinghe *et al.* (2021, p. 1856), reproduced here as Table 8.1, summarizes the AR6 assessment of whether a signal of attributable anthropogenic influence has emerged across all major CIDs. One of the themes of this chapter is that attribution methods used by the IPCC tend to overstate the anthropogenic influence and understate the role of natural variability. Nonetheless a striking feature of that summary table is how few CIDs exhibit an anthropogenic signal sufficient to distinguish them from natural variability.

Out of the 33 weather impact categories, an anthropogenic signal is asserted with *high confidence* in only five, and with *medium confidence* in a further four. (Note that one of the CIDs is an increase in CO₂ levels, and since it is a tautology to attribute this to increased CO₂ levels this CID can be ignored.) For the rest the IPCC does not claim to have detected anthropogenic drivers. Of the five *high confidence* assertions, two are for changes in average temperatures (air and ocean) hence are not measures of extreme weather. Further, two of the four *medium confidence* assertions are related to ocean chemistry and thus are likewise not related to extreme weather. The IPCC does not assert an attributable human influence on many non-temperature weather features such as wind, precipitation, flooding, drought etc.

Columns 2 and 3 of IPCC Table 12.12 report on whether anthropogenic signals are expected to emerge this century under RCP8.5, the most extreme forcing scenario. We have omitted these columns for several reasons. First, because they refer to whether climate models project detectable signals in the observations, which is a very different question than our concern here, whether a signal has been detected in historical data. Second, as we discuss in Chapter 4, the RCP8.5 scenario is a misleading and implausible high-end storyline, it is not a “base case” or business-as-usual projection. Third, the ensemble of detailed models is acknowledged (Palmer and Stevens 2019) to be “not fit for purpose” in describing regional changes and “unable to represent future conditions at the degree of spatial, temporal, and probabilistic precision with which projections are often provided.” (Nissan et al, 2019). Even with these caveats, we note that the omitted columns in AR6 Table 12.12 show most weather impacts are not expected to exhibit an anthropogenic signal in this century.

Climatic Impact-driver Type	Climatic Impact-driver Category	Already Emerged in Historical Period
Heat and Cold	Mean air temperature	1
	Extreme heat	2
	Cold spell	4
	Frost	
Wet and Dry	Mean precipitation	
	River flood	
	Heavy precipitation and pluvial flood	
	Landslide	
	Aridity	
	Hydrological drought	
	Agricultural and ecological drought	
	Fog weather	
Wind	Mean wind speed	
	Severe wind storm	
	Tropical cyclone	
	Sand and dust storm	
Snow and Ice	Snow, glacier and ice sheet	
	Permafrost	
	Lake, river and sea ice	11
	Heavy snowfall and ice storm	
	Hail	
	Snow avalanche	
Coastal	Relative sea level	
	Coastal flood	
	Coastal erosion	
Open Ocean	Mean ocean temperature	
	Marine heatwave	
	Ocean acidity	
	Ocean salinity	13
	Dissolved oxygen	14
Other	Air pollution weather	
	Atmospheric CO ₂ at surface	
	Radiation at surface	

High confidence of decrease

Medium confidence of decrease

Low confidence in direction of change

Medium confidence of increase

High confidence of increase

Table 8.1: Reproduction of column 1 of Table 12.12, IPCC AR6 Working Group I report. Emergence of anthropogenic signal in historical period for CIDs shown. White: no signal detected. Blue and orange: change detected (decrease or increase) and confidence level as indicated in color legend.

As discussed in Chapter 6 natural variability dominates patterns of extreme weather systems and simplistic assertions of trend detection are frequently undermined by regional heterogeneity and trend reversals over time. Table 8.1 makes the related point that, again contrary to popular belief, attribution of changes in most extreme weather types to human influence is not currently possible. Taking wind as an example, the IPCC claims that an anthropogenic signal has not emerged in average wind speeds, severe windstorms, tropical cyclones or sand and dust storms, nor is one expected to emerge this century even under an extreme emissions scenario. The same applies to drought and fire weather.

8.6 Extreme event attribution (EEA)

Examining the IPCC's treatment of extreme weather and climate event attribution more broadly, the AR6 provides an ambiguous assessment of the role of anthropogenic warming that differs between chapters. Chapter 11 of WGI states (Seneviratne *et al.*, 2021):

Evidence of observed changes in extremes and their attribution to human influence (including greenhouse gas and aerosol emissions and land-use changes) has strengthened since AR5, in particular for extreme precipitation, droughts, tropical cyclones and compound extremes (including dry/hot events and fire weather). Some recent hot extreme events would have been extremely unlikely to occur without human influence on the climate system.

By contrast, as noted above in Section 8.5, Chapter 12 of WGI (Table 12.12) paints a different picture – presumably, the expert judgment of different groups of authors for the two chapters came to different conclusions (Ranasinghe, 2021):

- High confidence in an increase in extreme heat events in tropical regions where observations allow trend estimation and in most regions in the mid-latitudes, medium confidence elsewhere
- Medium confidence in a decrease in extreme cold events in Australia, Africa and most of northern South America where observations allow trend estimation
- No evidence of emergence in the historical period of a change in river floods, heavy precipitation, drought, fire weather, severe wind storms, and tropical cyclones

While the overall issue of detecting changes in extreme weather events and their attribution remains ambiguous, most of the activity in this area relates to the attribution of particular extreme weather events. The most prominent effort is World Weather Attribution (WWA; worldweatherattribution.org), an international research initiative for extreme event attribution that purports to analyze how climate change influences the likelihood and intensity of extreme weather events. Their approach is to use large ensembles of regional climate models to compare an event in today's climate with one in a counterfactual pre-industrial climate without human influences.

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WWA has a prominent public presence in linking extreme weather to climate change, with its press releases attracting considerable attention in public and policy discussions. However, the WWA's extensive promotion of non-peer-reviewed findings, its funding ties, its open admission to shaping analyses to serve litigation, and its methodological challenges have sparked controversies, with critics questioning the robustness and impartiality of their conclusions (Pielke Jr. 2024). Despite these issues, WWA's work continues to influence climate science and media narratives. Technical criticisms of the approach include a lack of a formal detection process; an implicit assumption that 100 percent of the post-industrial warming is caused by greenhouse gases; and a failure to adequately account for internal climate variability.

Because EEA is relatively new, many basic methodological issues have yet to be settled in the expert literature. An important challenge is the lack of data. Extreme events are, by definition, rare. Many analyses of extreme event types (including the U.S. National Assessment Reports) only evaluate data since 1950 or 1970. However, as emphasized by Chapter 6, many of the worst extreme weather and climate events in U.S. history occurred prior to the first half of the 20th century, including in the early 19th century. And if paleoclimate reconstructions are considered, it becomes very difficult for an event to pass the detection thresholds of exceeding what is expected from natural variability, particularly if a reasonably sized geographic region is considered.

Another challenge is defining the event under study. There is a longstanding literature in statistics and econometrics on the challenge of analyzing data with outliers. The issue arises because a data series establishes a probability distribution defining the expected range of observations. If an outlier is observed it might indicate that the underlying process giving rise to the data distribution has changed (which in the weather context would mean that a climate change has been detected) or that the underlying process has multiple regimes each with a different probability distribution, in which case observing an outlier simply means we were temporarily in a different regime, but the system itself was unchanged. If a time series contains only a single outlier event at the end of the series it is not possible to determine which model is the correct one (Chen and Liu 1993). For instance, there might be an “ordinary” weather regime that yields a distribution of summer daytime highs in a particular coastal region, and a second “heatwave” regime that kicks in when an inland blocking event occurs, which yields a temperature distribution centered 15°C higher than the first one. A day with temperatures 13°C above normal would either be an extreme heat anomaly under the first regime or a somewhat cool event under the second, and we have no way in this case of knowing on statistical grounds which view is correct.

Visser and Petersen (2012) and Sardeshmukh *et al.* (2015) both point out that different distributions may fit observed data equally well but yield very different implications about the likelihood of a specific weather event. Visser and Petersen argue that, in view of the deep uncertainties of extreme weather analysis, drawing a connection between individual events and global climate change should be avoided. Furthermore, the existence of an outlier at the end of a data series poses the problem that estimates of the event probabilities will be biased whether the outlier is included or excluded (Barlow *et al.*, 2020). Methods to eliminate the bias have not yet been established, leading some experts (e.g. Miralles and Davison 2023) to argue that in settings in which a data series contains a single extreme event at the end, estimation of a return period for the extreme event will be so biased and uncertain that it should be avoided altogether.

We provide a case study of a recent high impact extreme event in the U.S. to illustrate the challenges and ambiguities in attributing the frequency and intensity of extreme weather events to human-caused warming.

8.6.1 Case study – 2021 Western North America heat wave

The 2021 Western North America heat wave was an extreme event that affected much of Western North America in late June 2021. In particular, surface temperature records were set in Portland, OR (116°F; previous record 107°F) and Seattle, WA (108 °F; previous record 103°F) (Mass *et al.*, 2024).

The WWA team generated international headlines with their analysis, which provided the following attribution statements (WWA, 2021; Philip *et al.*, 2022):

- Based on observations and modeling, the occurrence of this heatwave was virtually impossible without human-caused climate change.
- The event is estimated to be about a one in 1000-year event in today’s climate.
- The event would have been at least 150 times rarer without human-induced climate change.
- This heatwave was about 2°C hotter than it would have been if it had occurred at the beginning of the industrial revolution (when global mean temperatures were 1.2°C cooler than today).

But an important counter to the first claim is that other researchers concluded from historical weather data that while a heat wave of the magnitude observed was indeed virtually impossible without anthropogenic climate change, it was also virtually impossible *with* climate change. Bercos-Hickey (2022) noted “these temperatures were virtually impossible under any previously experienced meteorological conditions, with or without global warming.” McKinnon and Simpson (2022) stated “the most likely explanation remains that the weather event itself was ‘bad luck.’” The 2023 Oregon Climate Assessment (Fleischman, 2023) concluded that the heat dome would have formed even without climate change and “There is no evidence that the highly unusual combination of weather features that drove the heat dome were made more likely by climate change, and climate models do not project an increase in the frequency of high-pressure ridges over the Pacific Northwest” (Fleischman, 2023, p. 49).

Mass *et al.* (2024) summarizes the proximate sequence of compound events leading to the heat wave. There was a record-breaking mid-tropospheric ridge over the Pacific Northwest, forced by a tropical disturbance in the western Pacific. This produced record-breaking mid-tropospheric temperatures, strong subsidence in the lower atmosphere, low-level easterly flow that produced downslope warming on regional terrain and the removal of cooler marine air, and an approaching low-level trough that enhanced downslope flow. The event occurred at a time of maximum solar insolation, and drier-than-normal soil moisture. Using a storyline approach, Mass *et al.* assessed that there is no trend in drought and dry soils in the Pacific Northwest; there is no evidence of global warming producing stronger ridges of high pressure, and no observed trend in heat waves or record temperatures in the region. They concluded that although anthropogenic warming might have contributed as much as 2°F to the magnitude of the event, there is little evidence of further amplification of the event from increasing greenhouse gases.

Bercos-Hickey *et al.* (2022) conducted a statistical analysis of a model-based attribution study of this heat wave. Because the event is a far outlier and far above the bounds of Generalized Extreme Value distributions fitted from historical data, they concluded that estimates of return times, quantitative changes in event magnitude and frequency, and probability of the extreme temperatures such as provided by the WWA are not accurate and should be interpreted as having low confidence. They found that hindcast attribution methods using an ensemble of regional climate models, combined with Pearl (2009) causal inferences, can provide limited and conditional information about the magnitude of the human influence on this heatwave - they provided a more highly constrained estimate that human activities caused a ~1.4°F–1.8°F increase in the daily maximum temperatures.

Zeder *et al.* (2023) also concluded that the methods employed by Philip *et al.* (2022, the WWA analysis) tend to overstate the rarity of extreme heat waves, leading to a biased perception of the effect of climate change on the heatwave event: “The tendency to overestimate the return period of observed extreme heatwave events may fuel the impression that seemingly impossible heatwave extremes are currently clustering at an unprecedented rate.”

The 2021 Western North America heatwave was a rare and unprecedented compound weather event that broke century-old temperature records by as much as 9°F. While the WWA team received worldwide publicity for their rapid attribution claims blaming anthropogenic climate change, subsequent peer-reviewed analyses showed that the event was caused by rare meteorological conditions that were not made more probable by global warming.

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PART III: IMPACTS ON ECOSYSTEMS AND SOCIETY

9 CLIMATE CHANGE AND U.S. AGRICULTURE

Chapter summary

There has been abundant evidence going back decades that rising CO₂ levels benefit plants, including agricultural crops, and that CO₂-induced warming will be a net benefit to U.S. agriculture. The warming since 1981, in combination with adaptive behavior by farmers, has been beneficial for maize. The increased ambient CO₂ has also boosted productivity of all major U.S. crop types. Various attempts to argue that CO₂-induced warming will harm agriculture do not hold up under scrutiny.

9.1 Econometric analyses

Econometric analyses of the effects of climate change on agriculture have sought to integrate information about long term crop yield changes in response to temperature and precipitation changes under assumptions about adaptive behavior by farmers. One method focuses on variations in agricultural land values. The rationale is that if climate change is a long-term net benefit for agriculture it should be capitalized into higher market values for agricultural land, and vice versa. While individual crops might benefit from or be harmed by climate change, once the adaptive responses of farmers are considered the value of farmland represents an index of whether the changes are expected to be beneficial or not. Mendelsohn *et al.* (1994) examined the relationship between historical climatic variations on agricultural land values and concluded global warming would be slightly beneficial to American agriculture.

The Mendelsohn *et al.* method, called Ricardian analysis after David Ricardo, the 19th century British economist who pioneered the study of land values, attracted subsequent criticism from authors who argued it failed to account for differences in land values attributable to fixed locational characteristics like soil quality, and non-climatic changes including nearby urbanization. Deschênes and Greenstone (2007) looked at agricultural profits instead of land values and reached conclusions similar to Mendelsohn *et al.* (1994), namely that past climatic variations had relatively little effect on farm profitability and that warming would likely yield small overall benefits for the U.S. agricultural sector. However, a subsequent exchange with critics led them (Deschênes and Greenstone 2012) to revise their conclusions and project potentially large losses in U.S. agriculture due to climate warming.

Burke and Emerick (2016) looked at temperature variations over 1980 to 2000 and argued that farmers were not as able to adapt to temperature changes as the Ricardian method assumes and that climate change would have large negative impacts on corn and soy yields. Schlenker and Roberts (2009) similarly argued that yield gains to past warming would not carry over to the future and corn and soy yields would sharply decrease this century due to climate change.

Two recent studies have argued that pessimistic findings such as these are not robust. Ortiz-Bobea (2019) argued that land values aggregate farm and non-farm influences and the latter need to be filtered out. He developed a data set using cash rents for agricultural activity as a measure of land value specifically for farming activity. Whereas the land value model implied future losses under climate warming, the same model estimated using cash rents did not, leading the author to conclude the pessimistic results were due to using an inaccurate measure of the returns to farming activity. Bareille and Chakir (2023) assembled a large data base on farm sale prices in France for properties that sold twice between 1996 and 2019. They could replicate pessimistic results showing negative effects of warming on agricultural land values using conventional econometric modeling. But by taking advantage of the repeat sales data, which provides information on site-specific changes in land prices, they found the results reversed and implied that climate change will be very beneficial for French agriculture. The authors concluded that, taking adaptation into account, a warming climate would yield positive benefits for French agriculture that were between two and

20 times larger than had previously been estimated. On average, with full adaptation, they concluded that climate changes under the medium RCP4.5 scenario could double the value of French farmland by 2100.

A major deficiency of all these studies, however, is that they omit the role of CO₂ fertilization. Climate change as it relates to this report is caused by GHG emissions, chiefly CO₂. The econometric analyses referenced above focus only on temperature and precipitation changes and do not take account of the beneficial growth effect of the additional CO₂ that drives them. As explained in Chapter 2, CO₂ is a major driver of plant growth, so this omission biases the analysis towards underestimation of the benefits of climate change to agriculture.

9.2 Field studies of CO₂ enrichment

One of the ways the effect of CO₂ on crop growth has been studied is through “free air enrichment experiments” or FACE plots, in which small sources of CO₂ are placed in fields surrounding plants and the growth response to elevated CO₂ under varying weather conditions are recorded. Ainsworth *et al.* (2020) summarizes results from about 250 such studies. They found that elevation of CO₂ by 200 ppm caused an average 18 percent increase in crop yield in C3 plants. C4 plants exhibited benefits mainly under drought conditions.

In addition to FACE plot experiments there have been thousands of laboratory experiments on the effects of CO₂ on all kinds of plant growth. Here we review some of the results on key U.S. agricultural crops.

Soybean

Studies on the impact of elevated CO₂ on Soybean (*Glycine max*(L.) Merr.) plants in the water-deficient region of Huang-Huai-Hai Plain, China, showed that elevated CO₂ concentrations improved photosynthesis rate, water use efficiency, and growth Li (2013) under both normal conditions and drought conditions. The CO₂Science.org website reports on 108 published experiments between 1985 and 2019 exposing soybean to enriched CO₂ levels. Converted to a +300ppm common scale the average growth benefit was +50.9 percent. There were also ten studies reporting on +600 ppm CO₂ enrichment, which increased photosynthesis by an average of 90.3%.

Maize (corn)

The CO₂Science.org website reports on 28 published experiments between 1983 and 2018 exposing corn (*Zea mays* L.) to enriched CO₂ levels. Converted to a +300ppm common scale the average growth benefit was +23.7 percent. Corn also benefits from increased drought tolerance under elevated CO₂. An experimental study (Allen Jr., 2011) exposed plants to water stress conditions in sunlit controlled-environment chambers at 360 ppm (ambient) and 720 ppm (elevated) CO₂. The drought stress caused a 41 percent loss of growth under ambient CO₂ but only a 13 percent loss under elevated CO₂.

Wheat

The CO₂Science.org website reports on 92 published experiments between 1983 and 2020 exposing common wheat (*Triticum aestivum* L.) to enriched CO₂ levels. Converted to a +300ppm common scale the average growth benefit was +67.6 percent. Blandino (2020) measured both the yield and nutritional quality of an “improver” hybrid wheat variety and its parents under elevated CO₂ levels (+166 ppm). They reported a grain yield increase of +16 percent but a 7 percent decrease in grain protein levels. But they found that the food quality of different wheat varieties responded differently to elevated CO₂ levels, so that with proper varietal selection, growers could select wheat types that best take advantage of the elevated CO₂.

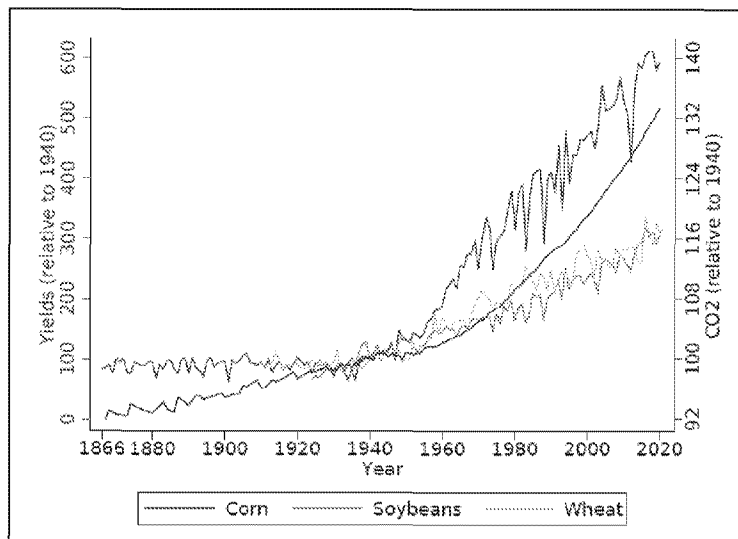


Figure 9.1: U.S. average CO₂ levels and yields of corn, soy and wheat all normalized so 1940=100. Source: Taylor and Schlenker (2021)

Further evidence

A 2021 report from the U.S. National Bureau of Economic Research (Taylor and Schlenker 2021) used satellite-measured observations of outdoor CO₂ levels across the United States, matched to county-level agricultural output data and other economic variables. After controlling for the effects of weather, pollution and technology the authors concluded that CO₂ emissions had boosted U.S. crop production since 1940 by 50 to 80 percent, attributing much larger gains than had previously been estimated using FACE experiments. They found that every ppm of increase in CO₂ concentration boosts corn yields by 0.5 percent, soybeans by 0.6 percent, and wheat by 0.8 percent.

Beyond growth benefits, extra CO₂ boosts plant resilience to dryness. Deryng *et al.* (2016) surveyed evidence on crop water productivity (CWP, the yield per unit of water used), drawing attention to the potential for CO₂ both to enhance photosynthesis and reduce leaf-level transpiration (water loss during leaf respiration). They surveyed all available FACE data on crop yield changes for maize (corn), wheat, rice and soybean, and combined it with crop model data simulating yield responses as of 2080 under the extreme RCP8.5 scenario in four growing regions (Tropics, Arid, Temperate and Cold) each of which were split into rainfed and irrigated sub-regions. In every region they reported that models without CO₂ fertilization predicted CWP losses but CO₂ fertilization more than offset them; once fertilization was taken into account, all regions showed a net CWP gain. Deryng *et al.* also reported that negative yield impacts on wheat and soybean were fully offset by CWP gains and mitigated by up to 90 percent for rice and 60 percent for maize. Similarly, Cheng *et al.* (2017) noted that increased global plant growth from 1982 to 2011 due to rising CO₂ uptake was accompanied by such large gains in CWP that despite the extra biomass global water use by plants had not increased.

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Deryng *et al.* (2016) assumed that climate change would “exacerbate water scarcity”. It is noteworthy that while models predict drylands will expand under climate warming, observations show the opposite: greening is happening even in arid areas. Zhang *et al.* (2024) report that due to increased CO₂ levels

“increasing aridity in drylands won’t lead to a general loss of vegetation productivity”; increased desertification will happen in at most 4 percent of currently arid areas.

9.3 Crop modeling meta-analyses

Notwithstanding the abundant evidence for the direct benefits of CO₂ and of CO₂-induced warming on crop growth, in 2023 the U.S. Environmental Protection Agency (EPA 2023) boosted its estimate of the Social Cost of Carbon (SCC) about five-fold based in large part on a very pessimistic 2017 estimate of global agricultural damages from climate warming (Moore *et al.*, 2017). One of the two damage models used by the EPA attributed nearly half of the 2030 SCC to projected global agricultural damages based on the Moore *et al.* (2017) analysis. This study was a meta-analysis of crop model studies simulating yield changes for agricultural crops under various climate warming scenarios. Moore *et al.* projected declining global crop yields for all crop types in all regions due to warming.

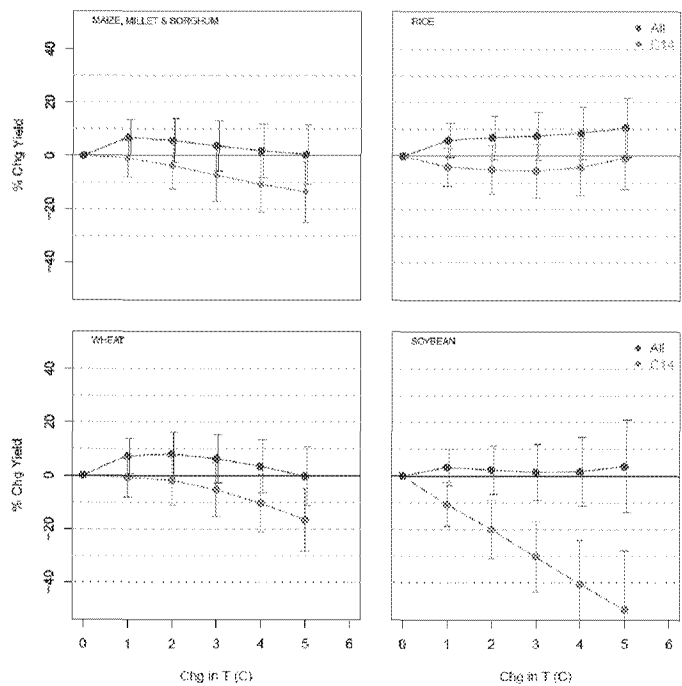


Figure 9.2 Crop yields under CO₂-induced climate warming. Blue: as published in Moore *et al.* (2017). Green: after including omitted data. Source: McKittrick (2025).

McKittrick (2025) re-examined the Moore *et al.* database and found that, while it claimed to cover 1,722 studies, only half the entries ($N=862$) had complete records, so that the sample available for regression analysis was much smaller than both [CLARIFY WHAT BOTH REFERS TO] studies indicated. McKittrick noted that the records most commonly missing were the changes in ambient CO₂ and found that in many cases these could be recovered from the underlying studies or the original climate scenario tables.

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He thereby increased the usable sample size by 40 percent. The crop yield projections incorporating the newly available data changed considerably. As shown in Figure 9.2, whereas the partial data set implied warming would decrease yield (blue lines), the complete data set implied constant or increased global yields, even out to 5°C warming (green lines).

In summary, there is abundant evidence going back decades that rising CO₂ levels benefit plants including agricultural crops, and that CO₂-induced warming will be a net benefit to U.S. agriculture.

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10 MANAGING RISKS OF EXTREME WEATHER

Chapter summary

Trends in losses from extreme weather and climate events are dominated by population increases and economic growth. Technological advances such as improved weather forecasting and early warning systems have substantially reduced losses from extreme weather events. Better building codes, flood defenses, and disaster response mechanisms have ~~lowered-reduced~~ economic losses relative to GDP. The U.S. economy's expansion has diluted the relative impact of disaster costs, as seen in the comparison of historical and modern GDP percentages. Heat-related mortality risk has dropped substantially due to adaptive measures including the adoption of air conditioning, which relies on the availability of affordable energy. U.S. mortality risks even under extreme warming scenarios are not projected to increase if people are able to undertake adaptive responses.

10.1 Socioeconomic context

Risks from human-caused climate change are affected by natural weather and climate variability and are dominated by the exposure of wealth in coastal and other disaster-prone regions and by vulnerabilities of poorer populations. The evolution of climate risk in the U.S. has been dominated by societal factors, rather than by changes to the actual weather and climate hazards. Deaths from weather disasters have decreased substantially since 1900, even as U.S. population grew from 76 million in 1900 to over 331 million in 2020 (Goklany 2011). For example, the Galveston hurricane killed over 8,000 people in 1900 (0.01 percent of the U.S. population), whereas the worst recent disaster, Hurricane Katrina in 2005, killed 1800 people (or 0.0006 percent of the U.S. population) (NOAA National Hurricane Center 2025; U.S. Census Bureau 2025).

Technological advances have substantially reduced losses from extreme weather events. Early warning systems, satellite monitoring, and improved weather forecasting have reduced deaths, although exact numbers are hard to quantify (Deryugina and Hsiang 2023). U.S. weather forecasting has been estimated to reduce losses from weather events with an annualized benefit of \$31.5B, protecting lives, property, and supporting agriculture and transportation (NRC 2010). Improved hurricane forecasts have reduced pre- and post-landfall spending, with annual per hurricane cost reductions estimated at \$5B (Molina and Rudik 2024).

Infrastructure improvements have contributed to substantial reductions in losses from extreme weather events. Building codes, such as those implemented in Florida after Hurricane Andrew (1992), have reduced losses by ensuring structures can withstand high winds and floods. Homes built after 2002 showed minimal damage during Hurricane Michael (2018), unlike older homes (FEMA 2020). Sea walls, like the Galveston Seawall, protect against wave action and storm surge. The New Orleans Hurricane and Storm Damage Risk Reduction System successfully mitigated storm surge during Hurricane Isaac (2012) (Battelle Memorial Institute 2013). Inland dams in the U.S. help control flooding by storing excess water during heavy rains. It is estimated that the Tennessee Valley Authority (TVA) dams prevent about \$309M in annual flood damage in the TVA region and along the Ohio and Mississippi Rivers (TVA, 2025). During Hurricane Helene (2024), TVA's strategies prevented approximately \$406M in potential damages (APPA 2024).

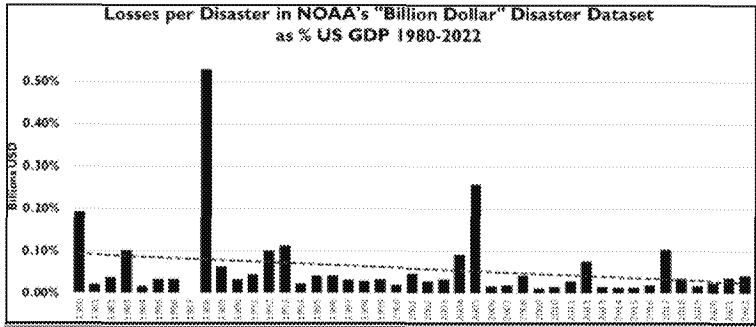


Figure 10.1: Losses per disaster in NOAA's billion-dollar disaster dataset (the version downloaded in July 2023), 1980 to 2022. Source: (Pielke, Jr. 2024)

10.2 Data challenges

Since 1980, NOAA has provided a count of annual U.S. weather-related disasters that it estimates to have exceeded \$1 Billion (inflation adjusted), showing a substantial increase starting in 2008. NOAA and other government officials have cited the upward trend in the Billion Dollar Disaster series as evidence that climate change is making extreme weather worse (Pielke Jr., 2024). But over time, population and wealth have increased dramatically in the U.S., so when an extreme weather or climate event occurs, there is more damage even if there is no underlying trend in the frequency or intensity of extreme weather. Pielke Jr. (2024) demonstrates that losses per weather disaster as a proportion of GDP have decreased by about 80 percent since 1980, as shown in Figure 10.1. Pielke Jr. (2024) also argued that in addition to relying on opaque data sources and unreported adjustments, NOAA failed to normalize its Billion Dollar Disaster data series properly for changes in population exposure and wealth. In May 2025 NOAA announced it has withdrawn the Billion Dollar Disaster product from publication (Pielke Jr., 2025).

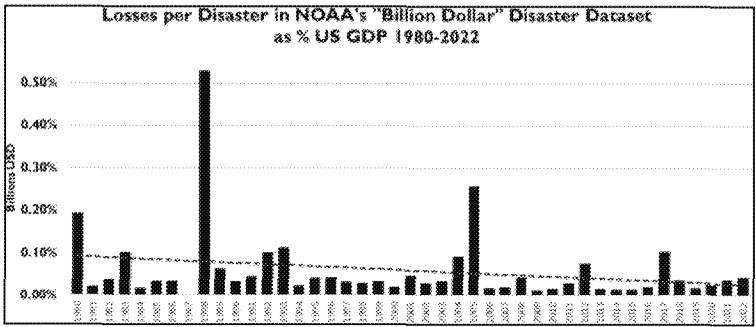


Figure 10.1: Losses per disaster in NOAA's billion-dollar disaster dataset (the version downloaded in July 2023), 1980 to 2022. Source: (Pielke, Jr. 2024)

In summary, trends in economic losses from extreme weather and climate events are dominated by population increases and economic growth. Technological advances such as improved weather forecasting and early warning systems have substantially reduced losses from extreme weather events. Better building codes, flood defenses, and disaster response mechanisms have lowered economic losses relative to GDP. Finally, the U.S. economy's expansion has diluted the relative impact of disaster costs, as seen in the comparison of historical and modern GDP percentages.

10.3 Mortality from temperature extremes

10.3.1 Heat and cold risks

Changes in temperature extremes are among the most certain impacts expected in a warming world. It stands to reason that extreme heat events would likely become more frequent while extreme cold events would become less frequent. This pattern is evident in the historical period though not in the continental U.S. (Chapter 6) and is expected to continue with further warming.

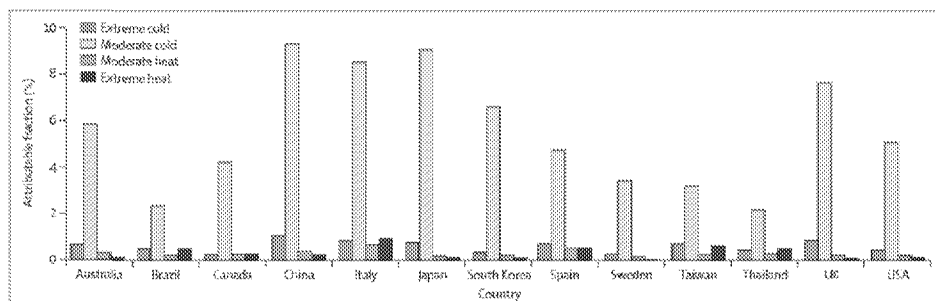


Figure 10.2 Mortality attributable to extreme and moderate cold and heat by country. Source: reproduced from Gasparini *et al.* (2015).

Mortality during heat extremes is typically caused by heat stroke and heat exhaustion, while mortality during cold extremes typically stems from hypothermia and heart strain. Global mortality is substantially greater for cold conditions than for hot conditions (Zhao *et al.* 2021, Ritchie 2024). Unlike with heat-related mortality cold-weather risks set in even at moderately cold conditions (Gasparini *et al.* 2015, Lee and Dessler 2023). The U.S. EPA (citing data from the Centers for Disease Control) reports that on average over 1999 to 2015 there were 2.2 deaths per million Americans for which cold was listed as the main underlying cause and an additional 2.4 deaths per million for which cold was listed as a contributing factor (EPA 2025). By contrast there were 1.3 deaths per million for which heat was the main cause and an additional 0.8 per million for which it was a contributor. By this metric cold accounts for approximately double the weather-related deaths as does heat.

Epidemiological methods, which consider correlational evidence, not just death certificate reports, indicate the cold/warm ratio may be much higher. A 13-country study of 74 million deaths from 1985 to 2012 estimated that, on average, 7.7 percent of deaths were attributable to sub-optimal temperatures, of which 7.4 percent were attributable to cold and only 0.4 percent were attributable to heat (Gasparini *et al.* 2015). In other words, cold weather killed 18.5 times as many people as did hot weather.

Figure 10.2 shows the distribution of results from Gasparini *et al.* (2015) by country. For the United States the fraction of deaths attributable to temperature was 5.9 percent, of which 5.5 percent was due to cold, thus cold weather killed 14 times as many people as hot weather.

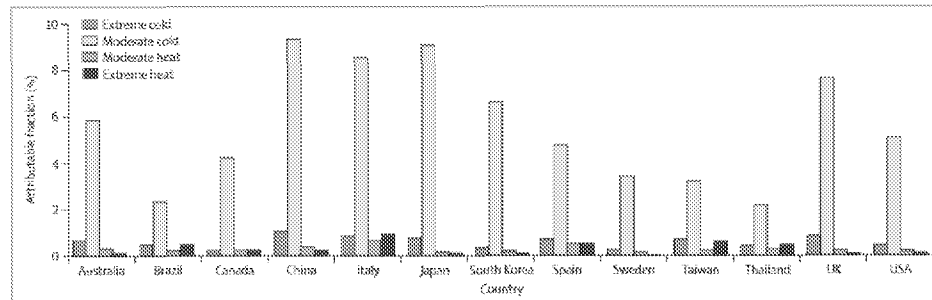


Figure 10.2. Mortality attributable to extreme and moderate cold and heat by country. Source: reproduced from Gasparini *et al.* (2015).

There is strong evidence that people adapt to weather risks. Lee and Dessler (2023) reported that 86 percent of temperature-related deaths across 40 cities in the U.S. were due to cold-related mortality, and that due to adaptation the relative risk of death declined in hot and cold cities alike as seasonal temperatures increased. Allen and Sheridan (2018) found that short, early-season cold events were 2 to 5 times deadlier than hot events, but the mortality risk of both cold and hot extremes drops to nearly zero if the events occur late in the season.

Davis *et al.* (2003) examined heat-related mortality in 28 U.S. cities from the 1960s to the end of the 1990s and found that heat-related mortality declined by three-quarters over the sample period. Bobb *et al.* (2014) examined mortality data for 106 million people in over 100 cities U.S. cities and found a 60 percent decline in average heat-related mortality over the period 1987–2005, from 51 per thousand deaths to 19. They furthermore found that the greatest drop was among seniors over the age of 75. In a study of 42 million deaths in 211 U.S. cities from 1962 to 2006, Nordio *et al.* (2015) found a more than 90 percent decline in the risk of mortality from excess heat.

In the context of large declines in heat-related mortality, by reducing mortality from cold events rising temperatures are associated with a net saving of lives. [DON'T UNDERSTAND THIS; REWORD]

The IPCC AR6 Working Group 2 Chapter 16.2.3.5 (O'Neill *et al.* 2022) acknowledges that heat-related mortality risk is declining over time:

Heat-attributable mortality fractions have declined over time in most countries owing to general improvements in health care systems, increasing prevalence of residential air conditioning, and behavioral changes. These factors, which determine the susceptibility of the population to heat, have predominated over the influence of temperature change.

It is both disappointing and telling to see how the IPCC misrepresents the situation in its AR6 Synthesis report. Section A.2.5 of that document states: “In all regions increases in extreme heat events have resulted in human mortality and morbidity (*very high confidence*).” But it is silent on the larger decline of deaths during extreme cold events.

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The observed decline in U.S. heat-related mortality has been specifically attributed to adaptation. Wang *et al.* (2018) exploited the spatial variability of heatwave-related mortality across 209 U.S. cities from 1962 to 2006. While simple correlation appeared to imply an increase in mortality risk during heatwaves, accounting for adaptation to heatwave intensity caused the effect to fall to near zero and become statistically insignificant. They used the results of their epidemiological model to project heat-related mortality out to 2050 under four RCP warming scenarios (including RCP8.5) with and without adaptive behavior. Assuming people continue to adapt to the heatwave risks in the regions in which they live, Wang *et al.* (2018) project not only no increase in heat-related mortality, but an overall decrease for the U.S. as a whole. They conclude that

Ignoring adaptation would result in a substantial overestimate of future mortality related to heat waves... Accounting for adaptation, the overall heat-related mortality by 2050 would not change substantially over time compared to 2006.

10.3.2 Mortality risks and energy costs

A 2016 study of U.S. long term mortality risks associated with temperature variations (Barreca *et al.* 2016) showed that increases in mortality across the U.S. are associated with both cold weather and hot weather. But over time, the introduction of electricity and the adoption of central heating and air conditioning (AC) dramatically reduced both risks, especially those associated with hot weather. Prior to 1960, a day above 90°F (32°C) added 2.2 percent to the average mortality risk rate, but after 1960 the same weather added only 0.3 percent to mortality risk, an 85 percent reduction. Prior to 1960 temperatures below 39°F (4°C) added about 1 percent to mortality risk, but after 1960 the same weather only added about half that amount. Adaptation through conventional household improvements dramatically reduced public vulnerability to weather extremes. The entire reduction in hot weather mortality was attributable to widespread adoption of indoor air conditioning (AC), which depended on the availability of reliable and affordable electricity.

The corollary of this finding is that the use of heating and cooling systems depends on energy being affordable. Doremus *et al.* (2022) showed that wealthy and poor households in the U.S. adjust their energy expenditures at similar rates in response to moderate temperature swings, but not in response to extreme temperature swings. When temperatures swing to very cold levels (< 5°C) energy spending in high-income households rises by 1.2 percent but in low-income households by only 0.5 percent. On very hot days (>30°C) electricity spending in high-income households rises by 0.5 percent but does not change at all in low-income households. The latter result is observed even in subsamples where all households have AC. The implication is that even with widespread adoption of home heating and cooling systems, inability to afford energy leaves low-income households more exposed to weather extremes.

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11 CLIMATE CHANGE, THE ECONOMY, AND THE SOCIAL COST OF CARBON

Chapter summary

Economists have long considered climate a relatively unimportant factor in economic growth, a view echoed by the IPCC itself in the AR5. Mainstream climate economics has recognized that CO₂-induced warming may have some negative economic effects but they are too small to justify aggressive abatement policies and that trying to “stop” or cap global warming even at levels well above the Paris target would be worse than doing nothing. An influential study in 2012 suggested that global warming would harm growth in poor countries, but the finding has subsequently been found not to be robust. Studies that take full account of modeling uncertainties either find no evidence of a negative effect on global growth from CO₂ emissions or find poor countries as likely to benefit from it as rich countries.

Social Cost of Carbon (SCC) estimates are highly uncertain due to unknowns in future economic growth, socioeconomic pathways, discount rates, climate damages, and system responses. The SCC is not intrinsically informative as to the economic or societal impacts of climate change. It provides an index connecting large networks of assumptions about the climate and the economy to a dollar value. Some assumptions yield a high SCC and others yield a low or negative SCC (i.e. a social benefit of emissions). The evidence for or against the underlying assumptions needs to be established independently; the resulting SCC adds no additional information about the validity of those assumptions. Consideration of potential tipping points at present does not justify major revisions to SCC estimates.

11.1 Climate change and economic growth

11.1.1 Overview: ~~climate is a relatively minor issue for a modern economy~~

It has long been noted that economies tend to do poorly in very cold and very hot regions, with the optimum somewhere in between (Nordhaus, 2006). Temperature-sensitive economic activity migrates, whenever possible, to where it is best suited, and society ~~always~~ adapts to the local climate. Based in part on these observations, in 1992 Thomas Schelling, then President of the American Economic Association, argued that any effects of climate change on U.S. economic activity would be small relative to the many other changes that would happen.

... Manufacturing rarely depends on climate, and where temperature and humidity used to make a difference, air conditioning has intervened. When Toyota chooses among Ohio, Alabama, and Southern California for locating an automobile assembly, geographical considerations are important, but not because of climate... Finance is little affected by climate; similarly for health care, or education, or broadcasting. Transportation can be affected, but improvements in all-weather landing and take-off in the last 30 years are greater than any differences that climate makes. If the average effect is a warming, iced waterways and snow removal may decline in importance. Construction is affected, mainly by cold, and if the average effect is in the direction of warming, construction may benefit slightly.

It is really agriculture that is affected. But even if agricultural productivity declined by a third over the next half century, the per capita GNP we might have achieved by 2050 we would achieve only in 2051. ...

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I conclude that in the United States, and probably Japan, Western Europe, and other developed countries, the impact on economic output will be negligible and unlikely to be noticed.

(Schelling 1992)

Thirty years later virtually the identical point was made by the IPCC itself in the Fifth Assessment Report:

For most economic sectors, the impact of climate change will be small relative to the impacts of other drivers. Changes in population, age, income, technology, relative prices, lifestyle, regulation, governance, and many other aspects of socioeconomic development will have an impact on the supply and demand of economic goods and services that is large relative to the impact of climate change.

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(Arent *et al.* 2014, emphasis added)

Evidence since AR5 does not change this assessment. Mohaddes et al. (2023) found that warm weather shocks have small negative effects on U.S. state-level output but not income, while cold weather shocks negatively affect both and the impacts are about four times larger, implying a shift to warmer conditions would, if anything, yield a net economic benefit for the U.S.

~~That These statements~~ is are validated by experience. Since 1900, the average global surface temperature anomaly warmed 1.3°C, about as much as the IPCC predicts will occur in the next century under a moderate emissions scenario. But even as the globe warmed and the population quintupled, humanity prospered as never before. For example, global average lifespan went from thirty-two years to seventy-two years, economic activity per capita grew by a factor of seven, and the death rate from extreme weather events plummeted by a factor of fifty!

While extreme weather events are costly, in all modern economies they are becoming steadily less and less important (Formetta and Feyen, 2019). Since 1990 weather-related disaster losses have declined as a proportion of global GDP (Pielke Jr, 2018, 2020) as have mortality risks (Formetta and Feyen, 2019). While economic weather-related insurance payouts are rising, this is fully explained by the growth in the size of the economy and the value of the insured asset stock. Past increases in episodes extreme weather have not had a significant effect (positive or negative) on the market value of insurance firms (Hu and McKittrick, 2015). Nor have past extreme weather events had a significant effect on U.S. banks' performance (Blickle *et al.*, 2021); warming has even been shown to be beneficial for the finance and insurance sector (Mohaddes et al. 2023). Figure 11.1 below is illustrative. For these reasons, economists have long been reluctant to endorse attempts to "stop" climate change or even aggressively reduce GHG emissions because the costs would not be worth it. As one critic of the economics of climate policy put it:

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Mainstream climate economic takes global warming seriously, but perplexingly concludes that the optimal economic policy is to almost do nothing about it... The contrast is striking. While climate science is sending out loud-and-clear messages that fossil-fuel disinvestment must start now, letting go of coal and oil and diverting resources into renewable energy technology systems, to keep warming below the 2°C limit (IPCC 2014), mainstream climate economics claims that overly ambitious climate targets will unnecessarily hurt the economy and immediate de-carbonization is too expensive. Most climate economists thus recommend humanity to just wait-and-see.

(Storm 2017)

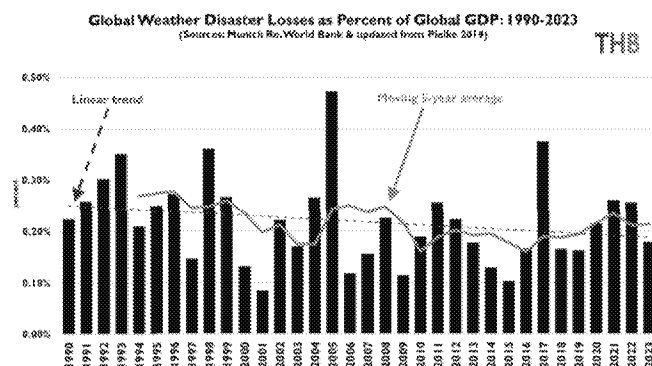


Figure 11.1 Global weather losses as a fraction of GDP. Source: Pielke Jr. (2023)

The mainstream economics position on the climate policy question is best represented by the findings over the past three decades from Integrated Assessment Models (IAMs) of climate change policy, for which Yale economist William Nordhaus was awarded the 2018 Nobel Memorial Prize in economics. IAMs combine economic, climate, and social data into a unified framework for simulating climate damages and evaluating the optimal policy response (Resources for the Future, 2025). Nordhaus' work has generally supported modest global climate policy, with aggressive measures largely deferred to later in the century. Nordhaus developed the so-called Dynamic Integrated Model of the Climate and Economy model, or DICE, in the early 1990s (Nordhaus, 1993) to study the interaction between climate change, climate policy, and global economic growth over long times. DICE assumes global emission control can be coordinated at no cost and asks what the optimal policy target should be. The climate component of the DICE model is based on simplified climatological modeling. The version of DICE at the time of Nordhaus' Nobel Prize award was described in Nordhaus (2018); it had a climate sensitivity parameter of 3.1°C for CO₂ doubling, consistent with the best estimate in contemporary IPCC reports (3.0°C).

The baseline (no policy) scenario incurs only \$0.4T in global abatement costs and leads to \$134.2T (trillion dollars) in global climate damages for a total cost of \$134.6T. JOVER WHAT TIME AND CAN WE SET A SCALE FOR THESE NUMBERS? GLOBAL ECONOMY/ The climate model component of DICE projects 4.1°C warming by 2100 relative to preindustrial temperatures. Note that this is a higher estimate of warming than many IPCC climate models.

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The Optimal Policy scenario barely deviates from the business-as-usual path. It aims for +3.5°C warming, in other words we modestly scale back fossil fuel use and otherwise just live with almost all the warming. This suggests that most CO₂ emissions are less harmful than the policies that would be necessary to abate them. Trying to prevent warming causes costs quickly to outstrip the benefits. Pursuing a goal of capping warming at 2.5°C creates total costs of \$177.8T, which is \$43.2T worse than doing nothing at all. Nordhaus didn't evaluate trying to reach a Paris-type target of 1.5°C warming but it would be even worse.

A subsequent edition of DICE includes higher assumed damages from warming which, not surprisingly leads to more aggressive policy recommendations, as explained in the section below on the Social Cost of Carbon.

11.1.2 Empirical analysis of climate change and economic growth

Many other studies have used econometric methods applied to historical data, instead of IAMs, to study the potential impact of climate change on economic growth. Dell *et al.* (2012, herein DJO12) was an influential study that used a multi-country panel of national-level data spanning 1950 to 2005, in which they matched climate and economic data by averaging temperature from the local grid cell level up to the national level using population weights. They found that warming yields an insignificant positive effect on income growth in rich countries but a significant negative effect in poor countries. Moore and Diaz (2015) modified the DICE model to take that finding into account and concluded that it implied a dramatically higher Social Cost of Carbon due to the compounding effects of income loss over time.

A large subsequent literature has debated the robustness of the DJO12 findings. Burke *et al.* (2015) analyzed a global panel with temperatures averaged up to the national level and found a negative effect on growth from warming in rich and poor countries alike when the national average temperature is above 13°C. Zhao *et al.* (2018) used the G-Econ data set of Nordhaus (2006), which breaks economic activity down to the grid cell level, replicated DJO12-type results on the same subset of countries as used in DJO12 but then showed that on the full global sample warming increases growth in rich countries and poor countries alike, though the positive effect in the latter group is confined to where local temperatures are below about 16°C. Greßer *et al.* (2021) developed a regional economic data set for 1,542 sub-national regions around the world between 2005 and 2015 and found temperature had no effect on growth. Yang *et al.* (2023) updated the DJO12 data set and applied an estimator robust to mixed sample frequencies, finding that while temperature shocks exerted a temporary effect on income levels, they did not have a significant lasting effect on growth rates.

Newell *et al.* (2021) noted that there is no underlying theory to guide econometric model specification in this literature. Taking into account the arbitrary choices of which explanatory variables to include they identified over 800 possible model specifications. Using the Burke *et al.* (2015) data they used an estimation method that accounts for model uncertainty and found that the model form preferred by Burke *et al.* (2015), which implied negative effects of warming on growth even in rich countries, is explicitly excluded by the optimal model selection algorithm. ~~Overall~~^{Overall}, they could not detect a temperature effect on GDP or GDP growth, and they estimated that the 95 percent confidence interval for the impact on global growth as of 2100 even under the exaggerated RCP8.5 warming scenario spans -86 percent to +388 percent. In other words, the net effect is likely positive but too uncertain to distinguish from zero.

Barker (2023) criticized the DJO12 assumption that countries can be grouped into fixed “poor” and “rich” categories based on their incomes many decades ago. He noted that many countries were once poor but became rich over time, and if this is considered, the original temperature effects reported by Dell *et al.* became small and insignificant.

Berg *et al.* (2023) argued that countries shouldn’t be grouped into large Rich/Poor categories because they are too heterogeneous. They instead estimated country-specific temperature response coefficients, then grouped countries with similar response coefficients into small panels. They separately estimated responses to global and idiosyncratic local temperature shocks to better identify the climate signal in weather data. Overall, they found countries experiencing negative effects of warming on growth outnumbered those experiencing positive effects, but only temporarily: eventually the effects reverse such that about twice as many countries experience a net positive growth effect. They also found global (as opposed to local) temperature changes are much more likely to benefit growth in poor countries than rich ones. In a simulation to 2100, even using the extreme RCP8.5 scenario, they computed the average global GDP loss would be

only 1.9 percent compared to a scenario with no warming. That is, instead of the economy growing 400 percent it would grow 392%. The implication of Nordhaus' earlier analysis is that trying to prevent the warming would lead to far less than 392 percent growth.

To summarize, economists consider climate a relatively unimportant factor in economic growth, a view echoed by the IPCC itself in the Fifth Assessment Report. Mainstream climate economics has recognized that CO₂-induced warming may have some negative economic effects but they are too small to justify aggressive abatement policy, and trying to “stop” or cap global warming even at levels well above the Paris target would be worse than doing nothing. An influential study in 2012 suggested that global warming would harm growth in poor countries but the finding has subsequently been found not to be robust (Tol 2024). Studies that take full account of modeling uncertainties either find no evidence of a negative effect on global growth from CO₂ emissions or find poor countries as likely to benefit from it as rich countries.

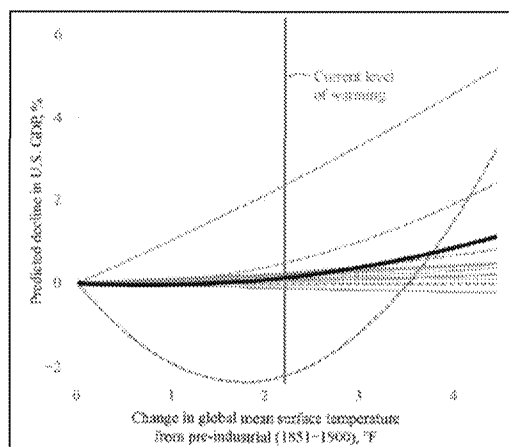


Figure 11.2: Decline in U.S. GDP per degree of warming. Source: CEA-OMB (2023)

The expectation that significant global warming would have a small impact on the U.S. economy was acknowledged quietly by the Biden Administration, even as the President was proclaiming a climate emergency. Figure 11.2, from a 2023 CEA-OMB report, shows the expected decrement in U.S. GDP as a function of temperature rise. The colored lines show the results of a dozen peer-reviewed published studies while the solid black line is their average. The figure could be summarized as “a few percent impact for a few degrees of warming”. Given that the economy’s annual growth rate is expected to be 1-2 percent, the impact of a warming globe is indeed negligible.

11.2 Models of the Social Cost of Carbon

The Social Cost of Carbon (SCC) is a tool for quantifying the economic impact of carbon dioxide emissions, helping policymakers weigh the costs and benefits of climate policies. It estimates the damage caused by emitting one additional ton of CO₂, expressed in dollars. More formally, the SCC is the discounted present value of the current and future marginal loss of economic welfare due to an additional ton of CO₂ entering the atmosphere.

11.2.1 Estimating the SCC

Although the literature refers to “estimates” of the SCC, it is not estimated in the way other economic statistics are estimated. For instance, data on market transactions including prices and quantities can be used to estimate the current inflation rate or the growth rate of per capita real Gross Domestic Product, and there are well-understood uncertainties associated with these quantities. But there are no market data available to measure many if not most of the marginal damages or benefits believed to be associated with CO₂ emissions, so these need to be imputed using economic models.

For example, an influential component of some SCC calculations is the perceived social cost associated with ~~changed the changed~~ risk of future mortality due to extreme weather. There is no market in which people can directly attach a price to that risk. At best economists can try to infer such values by looking at transactions in related markets such as real estate or insurance, but isolating the component of price changes attributable to atmospheric CO₂ levels is very difficult.

Economists use IAMs to compute the SCC. Two of the best known are the Climate Framework for Uncertainty, Negotiation and Distribution (“FUND”, Tol 1997) and Nordaus’ DICE. IAMs embed a “damage function” or set of functions relating ambient temperature to local economic conditions. The assumptions embedded in the damage function will largely determine the resulting SCC. IAMs also assume a long-term discount rate or, as in DICE, compute the optimal internal discount rate as part of the solution.

One approach to developing a damage function is to begin with estimates of the costs (or benefits) of warming in specific sectors in countries around the world and aggregate up to a global amount. This was the approach used in FUND. An alternative approach is to develop a simple equation that penalizes global income according to a simple quadratic function of the average global temperature. This approach was used in DICE. In the case of the FUND model many hundreds of parameters had to be selected, whereas in DICE only three were needed and were originally chosen to assign a predetermined penalty (1.2%) to global output as of 3°C warming, with a quadratic term implying that damages grow with the square of the global average temperature anomaly. Barrage and Nordhaus (2024) recently changed the parameters to increase the penalty at 3°C to ~~1.6%~~ and ~~1.6%~~ and added a discrete additional 1.0 percent GDP penalty at 3°C warming to account for “tipping points” (discrete large-scale environmental changes triggered by crossing a warming threshold) and a “judgmental adjustment” of 0.5 percent for excluded impacts at 3°C warming. Not surprisingly the newer version of DICE generates much higher SCC estimates than before.

The concepts of estimation and uncertainty do not readily apply to SCC calculations. By comparison, the question of whether precipitation has trended up or down along the South Carolina coast for the past hundred years can be addressed by collecting rainfall data, estimating a trend parameter and calculating statistical confidence intervals using methods with well-understood mathematical properties. In this way uncertainty can, in principle, be resolved by collecting more data and applying more accurate methods. But no amount of data collection can change the fact that many components of the SCC are unknown and rely on judgment and opinion based on knowledge of the underlying literature on the physical effects of climate change.

SCC calculations are thus best thought of as “if-then” statements: *if* the following assumptions hold, *then* the SCC is \$X per tonne. The list of “if” statements includes the premise that the world’s climate and economy work according to the representation in the IAM. One way this may fail to hold is in the timing of warming. Every IAM assumes a value of the Equilibrium Climate Sensitivity (ECS), which controls the amount of warming that results from CO₂ emissions and can be freely varied for the purpose of generating a distribution of SCC values associated with uncertainties over ECS. But as Roe and Baumann (2013) pointed out, time-to-equilibrium increases with the square of ECS, so an upward adjustment of the ECS parameter without an appropriate slowing down of the adjustment process can yield distorted present value damage estimates. In particular, the upper tail of warming associated with some commonly-used ECS distributions is physically impossible for even a thousand years into the future (Roe and Baumann 2013)

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yet in an IAM would be realized within a couple of centuries. Failure to align ECS with time-to-equilibrium will lead to an overestimate of the SCC value.

11.2.2 Variations in the SCC

Every level of the IAM calculation includes assumptions, some more influential than others. Key assumptions include the following.

- The discount rate: Climate damages accrue over a long time horizon and costs a century or two in the future need to be discounted to the present. The higher the discount rate the smaller the present value of future damages and vice versa. The discount rate represents the opportunity cost of spending money today rather than investing it and then having more to spend tomorrow. Some economists have argued for the use of very low discount rates in SCC calculations, resulting in policy recommendations that favor relatively large immediate investments in CO₂ emission reductions. The downside is that other investments could potentially earn a larger rate of return for society.
- Equilibrium Climate Sensitivity: IAMs have customarily employed a value of 3.0°C or 3.1°C following the IPCC's guidance. Empirically-estimated ECS values tend to be lower than this (see Chapter 9). Dayaratna *et al.* (2017, 2020, 2023) have shown that use of lower empirically-derived ECS values dramatically lower the resulting SCC estimate, even when low discount rates are used.
- Damage function coefficients: IAMs assume CO₂ and warming cause net harms that increase exponentially with temperature. More recently, IAMs have also incorporated effects from assumed potential climate tipping points. The FUND model took limited but explicit account of CO₂ fertilization effects in agriculture. Since the coefficients were selected ~~prior before~~ the publication of the current evidence of global greening and the magnitude of benefits to crops from elevated CO₂ (see Chapters 2 and 9), the growth effects are likely understated. The DICE model (and others) did not explicitly include any CO₂ fertilization benefit, except to the extent it was taken into account in the literature consulted when ~~picking choosing~~ the damage function coefficients. The damage function in FUND contains a region in which low warming yields net benefits in many regions, a finding which is supported by econometric models of warming and growth (Berg *et al.* 2023) and econometric simulations of agricultural changes (McKittrick 2025). The DICE damage function, by construction, rules out net benefits at any warming level.
- Emission scenarios: IAMs generate SCC estimates that increase as the pre-existing concentration of CO₂ increases. Consequently the value of damages later in the century will be higher, depending on the assumed baseline emissions over the coming decades.
- Abatement costs: IAMs represent the cost to the economy of reducing CO₂ emissions. If CO₂ emission reductions are assumed to be inexpensive, then the model will conclude that the optimal policy should aim for deeper emission cuts and vice versa.

It is informative to ascertain whether SCC results are invariant to changes in some assumptions. But when different assumptions result in higher or lower SCC values, the change in the SCC value does not provide *prima facie* evidence about the validity of the assumptions. For example, in 2023 the U.S. Environmental Protection Agency raised its preferred SCC value about 5-fold over the estimates it had issued ten years earlier. This is not because new data had been collected or better mathematical methods had been invented, but because new assumptions had been used, and the validity of those assumptions was a separate question. One was that global agricultural damages were far higher than previously assumed, based on an analysis in Moore *et al.* (2017). But as discussed in Chapter 9, McKittrick (2025) showed that Moore *et al.* (2017) had used a database in which half the CO₂ change observations were missing. When as many of those observations as possible were recovered from underlying sources and the analysis was rerun,

the projected crop yield losses disappeared and turned into gains at all warming levels. Hence the portion of the EPA's SCC revision attributable to agricultural yield losses was unwarranted.

11.2.3 Evidence for low SCC

Chapter 2 reviewed evidence on climate change and greening, and Chapter 9 looked at climate change and U.S. agriculture. Evidence shows that CO₂ fertilization has a stronger beneficial effect on agriculture than was known at the time that IAMs like DICE and FUND were parameterized. In particular, Haverd *et al.* (2020) found the observed CO₂ fertilization rate has been almost double what had been predicted by crop models. Dayaratna *et al.* (2023) used the updated empirical ECS distribution estimate of Lewis (2022), which assimilates modern instrumental and paleoclimatic temperature records, and allowed for a 30 percent gain in the CO₂ fertilization benefit in the FUND model, and found to find that, even at a low discount rate of 2.5 percent, the median SCC as of 2050 is only \$18.67, with a 24 percent probability of the true value being negative. At a five percent discount rate the median SCC value is negative until the mid-2040s and at 2050 was only \$0.37 with a 49 percent probability of being negative. Thus, under reasonable assumptions, a mainstream IAM using updated scientific inputs yields evidence consistent with the SCC not being significantly greater than zero.

It should also be noted that the SCC is focused on the social costs of CO₂ emissions from fossil fuel use. It does not measure the private marginal benefits to consumers and society from the availability of fossil fuels. Public willingness to pay for fuels of all types indicates the value to society of reliable, abundant fossil energy. Tol (2017) estimates that the private benefit of carbon is large relative to the social cost. This can be illustrated by noting that the price of a gallon of gas indicates the marginal value to the consumer of the fuel. Suppose we assume a relatively high Social Cost of Carbon of, say, \$75 per tonne. Deflated by a MCPF³ value of 1.5 that would result in a carbon tax of \$50 per tonne, which equates to about 44 cents per gallon of gas (Lavelle, 2019). A pre-tax price of \$3.00 per gallon would imply the marginal social benefit of the fuel is nearly seven times the marginal social cost.

11.2.4 Tipping points

SCC calculations typically consider gradual impacts of a warming climate, such as slowly-melting glaciers and steadily increasing average temperature. A driver of potentially high values of the Social Cost of Carbon (SCC) is the introduction into models of discrete catastrophic outcomes associated with abrupt changes (Dietz *et al.*, 2021). They are often referred to as “tipping points.”

The term “tipping point” mingles two different physical concepts that pose different research challenges. Many physical systems are inherently stable unless acted upon with sufficient external energy. For example, an ice cap might remain intact over a wide range of temperatures but once the temperature crosses the 0°C threshold it melts. Such discontinuities are ubiquitous in nature and require an external force. Whether the force needs to be large relative to the size of the system depends on the underlying stability of the system.

A different type of tipping point is called a *bifurcation* and arises from the study of the internal dynamics of nonlinear systems (Crawford, 1991). Many systems have been observed to have more than one equilibrium point and can move between them with minimal or no external influence. For example, a weather system might have two different equilibrium states: one calm and one with a tornado. A transition from one to another can happen either with no external force or with a minuscule change, such as a flap of

³ Marginal Cost of Public Funds: the optimal carbon tax rate is the SCC divided by the MCPF (Sandmo 1977).

the proverbial butterfly’s wings (Shen *et al.* 2014). The term “tipping point” is sometimes used to mean a bifurcation of this type and implies instability inherent in the system itself, which is not necessarily dependent on outside forces. It depends, instead, on parameters of the system taking values that support the emergence of bifurcations (Crawford, 1991).

The two different concepts imply different research questions. Regarding the first we are interested in whether components of the climate system are susceptible to abrupt discontinuities in response to sufficiently-large anthropogenic forcing. Regarding the second we are interested in whether the Earth’s climate system has inherent bifurcations that imply the possibility of abrupt transitions with or without external forcing.

Models have been developed that imply the ~~latter-second type~~ is a possibility. Kypke *et al.* (2020) presented a simple climate model in which the GHG concentration is one of the parameters that controls the emergence of bifurcations of the Arctic climate to one with both cold and warm equilibria. If sufficiently high GHG forcing combined with a sufficiently high rate of ocean heat transport are imposed a bifurcation becomes possible. More generally GCMs have been observed to contain bifurcations and multiple equilibria (Brunetti and Ragon, 2023).

The possible existence of bifurcations in the Earth’s climate system implies abrupt transitions are possible, not just in response to large forcing but also to small perturbations. This places tipping points into the category of low-likelihood and potentially catastrophic events, such as large meteor strikes. A key question to ask is whether those kinds of tipping points can be predicted. Current research has not resolved that question (Dakos *et al.*, 2024) and indeed may not be able to since one implication of the “butterfly effect” is the existence of predictability boundaries of nonlinear systems (Palmer *et al.*, 2014). It is therefore not obvious how to incorporate such possibilities into SCC calculations. Small variations in assumptions will lead to arbitrarily large variations in the resulting SCC with no grounds for choosing among them. If such tipping points are possible the most appropriate stance for economic policy is to maximize resilience to any form of external catastrophe since it is unlikely we could predict it or prevent it from happening.

The IPCC AR6 (Chapter 1) focuses mainly on the first type of tipping point, namely an abrupt change in response to external forcing. This is also the meaning associated with popular usage of the “tipping point” concept in discussions of climate change (see [[HYPERLINK "https://report-2023.global-tipping-points.org/what-is-a-tipping-point/"](https://report-2023.global-tipping-points.org/what-is-a-tipping-point/)] for example). As summarized by the IPCC AR6, there is evidence of abrupt change in the paleoclimate record, and some of these events have been interpreted as tipping points. Some projections with Earth System Models for example have produced tipping points such as Amazon forest dieback in response to specified values of CO₂ concentration or temperature increases.

The alarm surrounding climate tipping points is reflected by *The Global Tipping Points Report* that was launched at the ~~COP28~~COP28 on 6 December 2023 (Lenton *et al.*, 2023). It identifies more than 25 parts of the climate system said to constitute tipping points. What gets classified as a climate “tipping point” is a moving target. The most common examples in the literature and assessment reports include: Greenland ice sheet disintegration, West Antarctic ice sheet disintegration, summertime disappearance of Arctic sea ice, Amazon rainforest dieback, coral reef ~~die-off~~die-off, thawing of permafrost and methane hydrates, Atlantic Meridional Overturning Circulation collapse, boreal forest shift, West African monsoon shift, and Indian Monsoon shift.

All such tipping points require a certain amount of system instability ~~in order to~~to exhibit an abrupt transition in response to warming. For this ~~reason~~reason, there seems to be very little to discriminate between a tipping event and natural climate variability. Natural climate variability has in the past produced shifts in the West African and Indian monsoons, Amazon forest and coral reef dieback, and disintegration of ~~parts of~~the Greenland and West Antarctic ice sheets. These impacts can be reversed on the decadal ~~and century~~ timescales associated with natural climate variability and ecosystem responses.

Some abrupt changes are potentially more consequential, including collapse of the West Antarctic ice sheet and collapse of the Atlantic Meridional Overturning Circulation (AMOC). The IPCC AR6 WG1 Summary for Policy Makers states:

The Atlantic Meridional Overturning Circulation is very likely to weaken over the 21st century for all emission scenarios. While there is *high confidence* in the 21st century decline, there is only *low confidence* in the magnitude of the trend. There is *medium confidence* that there will not be an abrupt collapse before 2100. (C.3.4)

There is limited evidence for low-likelihood, high-impact outcomes (resulting from ice-sheet instability processes characterized by deep uncertainty and in some cases involving tipping points) that would strongly increase ice loss from the Antarctic Ice Sheet for centuries under high GHG emissions scenarios (e.g., SSP5-8.5). (B.5.2)

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~~For the purpose of~~ For SCC calculations, the research question implied by this type of tipping point is whether such events have been observed in the past in climate conditions similar to what we currently experience or will experience in the near future. The IPCC AR6 finds little evidence for impending collapse of the Atlantic Meridional Overturning Circulation or the West Antarctic ice sheet. It also finds there is no tipping point associated with Arctic Sea ice (AR6 Technical Summary p. 76)

Dietz *et al.* incorporated several potential tipping points (abrupt changes) into an SCC model and found they added about 25 percent to the estimate, mainly associated with thawing permafrost and release of methane hydrates. ~~However~~ however, the IPCC considers this scenario *very unlikely* (AR6 Technical Summary p. 107).

In summary, there may be unknown bifurcation tipping points that are associated with natural climate processes, but this possibility does not translate into specific guidance on the SCC. There are potential abrupt change points in the climate system in response to warming, although the IPCC assigns low probabilities to most, including the largest ones. When these have been ~~taken into account~~ considered, the result is only a modest increase in the SCC value in the 21st century.

11.2.5 Are there alternatives?

It is increasingly being argued that the SCC is too variable to be useful for policymakers. Cambridge Econometrics (Thoung, 2017) stated it's "time to kill it" due to uncertainties. The UK and EU no longer use SCC for policy appraisal, opting for "target-consistent" carbon pricing (UK Department for Energy Security and Net Zero 2022, Dunne 2017). However, the uncertainty of SCC estimates doesn't mean that other regulatory measures are inherently better or more efficient. Many emissions regulations (such as electric vehicle mandates, renewable energy mandates, energy efficiency regulations and bans on certain types of home appliances) cost far more per tonne of abatement than any mainstream SCC estimate, which is sufficient to establish that they fail a cost-benefit test.

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12 GLOBAL CLIMATE IMPACTS OF U.S. EMISSIONS POLICIES

Chapter Summary

U.S. policy actions are expected to have undetectably small direct impacts on the global climate and any effects will emerge only with long delays.

12.1 The scale problem

The emissions rates and atmospheric concentrations of criteria air contaminants are closely connected because their lifetimes are short and their concentrations are small; when local emissions are reduced the local pollution concentration drops rapidly, usually within a few days. But the global average CO₂ concentration behaves very differently, since emissions mix globally and the global carbon cycle is vast and slow. Any change in local CO₂ emissions today will have only a very small global effect, and only with a long delay.

Following the emission of a pulse (release) of CO₂ into the atmosphere, only about 40 ± 15 percent of the emitted CO₂ will have been sequestered after twenty years. That fraction rises to 75 ± 10 percent after a thousand years, and the remainder will be gradually removed over the ensuing tens of thousands of years (Ciais *et al.*, 2013, pp. 472-473). Consequently, any reduction in U.S. emissions would only modestly slow, but not prevent, the rise of global CO₂ concentration. And even if global emissions were to stop tomorrow, it would take decades or centuries to see a meaningful reduction in the global CO₂ concentration and hence human influences on the climate.

Reducing the atmospheric stock of CO₂ would require emissions to fall below the natural sequestration rate, assuming the entire increase is anthropogenic. Since that rate has been averaging about 50 percent of emissions in recent decades, a reduction of global emissions by 50 percent would (at least temporarily) halt the rise in atmospheric CO₂. The 1997 Kyoto Protocol proposed to cap industrial nations' CO₂ emissions at a modest five percent below 1990 levels by the year 2012. Even though this policy was too difficult for most nations to implement, full compliance would not have substantially reduced atmospheric CO₂ levels. It would only have slightly slowed CO₂ growth, reaching the projected year 2100 level in 2105 instead (Wigley, 1998). Lomborg (2016) estimated that full compliance with the Paris Accord would not stop warming, it would only prevent about 0.1C warming and delay hitting the baseline year 2100 temperature levels by about a decade.

Thus, in contrast with conventional air pollution control, even drastic local actions will have negligible local effects, and only with a long delay. The practice of referring to unilateral U.S. reductions as “combating climate change” or “taking action on climate” therefore reflects a profound misunderstanding of the scale of the issue.

12.2 Case study: U.S. motor vehicle emissions

The scale problem can be illustrated with reference to U.S. motor vehicles. The EPA's 2009 Endangerment Finding focused on CO₂ emissions from cars and light-duty trucks in the U.S. because Section 202(a) of Clean Air Act mandates the EPA to set emissions standards for motor vehicles if pollutants are found to endanger public health or welfare. The 2009 Endangerment Finding therefore obligated the EPA to regulate emissions from new motor vehicles, ostensibly to reduce or eliminate climate-related harms to the U.S. public.

Two questions that naturally arise are: (1) How large a reduction in CO₂ emissions would result from such regulation? and (2) What would be the climate impact of such regulation?

The first question can be addressed by comparing U.S. vehicle-based CO₂ emissions to the global total. The second question can be addressed by using the fact that the reduction in global warming would be, according to the models relied upon by the EPA, proportional to the reduction in global emissions, keeping in mind that the change in the CO₂ content of the atmosphere in any given year is the result of total global CO₂ emissions, not just U.S. emissions.

In 2022, the emissions from U.S. cars and light duty trucks totaled 1.05 billion metric tons of carbon dioxide (GtCO₂, EPA 2024). Meanwhile global CO₂ emissions from energy use totaled 34.6 GtCO₂ (Energy Institute 2024). Hence U.S. cars and light trucks account for only 3.0 percent of global energy-related CO₂ emissions. To a first approximation we can say that even eliminating all U.S. vehicle-based emissions would retard the accumulation of CO₂ in the atmosphere by a year or two over a century.

It would also reduce the overall warming trend by at most about 3 percent. For the period 1979-2023, which has the most extensive global coverage of a variety of weather data types, warming trends are determined to a precision of about ± 15 percent, so the impact of reducing the rate of global warming by eliminating U.S. vehicle CO₂ emissions would be far below the limits of measurability. Given that global-average temperature is the most direct climate change metric, impacts on any secondary climate metrics (e.g. severe weather, floods, drought, etc.) from reducing U.S. vehicle CO₂ emissions would be even less measurable.

Consequently, in contrast to the case of local air contaminants like particulates and ozone, even the most aggressive regulatory actions on GHG emissions from U.S. vehicles cannot be expected to remediate alleged climate dangers to the U.S. public on any measurable scale.

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GLOSSARY

2XCO₂: Doubling of the atmospheric carbon dioxide (CO₂), a commonly used baseline for addressing climate change. In 2025, atmospheric CO₂ concentrations were approximately 50 percent of the way to 2XCO₂ compared to 1800.

ABATEMENT: In the context of environmental policy, the reduction of emissions.

ACE (Accumulated Cyclone Energy): A statistical measure of a tropical cyclone's energy accumulated over its lifetime, calculated from a summation of the square of maximum sustained wind speeds.

ACIDIFICATION: A commonly used term for the reduction in ocean pH from more alkaline to less alkaline values.

ACRIM (Active Cavity Radiometer Irradiance Monitor) GAP: the gap in total solar irradiance data between 1989 and 1991 caused by a delay in the launch of a satellite-borne monitor in time to overlap and intercalibrate with the prior system.

AEROSOLS: Tiny solid or liquid particles suspended in the air.

ALKALINE: Having a chemical pH greater than 7.0. The "opposite" of acidic, which means a pH less than 7.0.

AMBIENT: Relating to the immediate surroundings of something.

AMO: Atlantic Multidecadal Oscillation, a 60-year cycle of sea surface temperatures in the North Atlantic.

ANTHROPOGENIC: Created by humans.

AR (Assessment Reports): Periodic assessments published by the Intergovernmental Panel on Climate Change evaluating current knowledge of the climate system, global change, and associated issues. An AR now consists of reports from three working groups, the first (WG1) concerned with physical science and the others concerned with societal impacts and mitigation strategies. The most recent was the AR6.

ATTRIBUTION: Assertion of a causal relation, chiefly from anthropogenic GHG emissions to observed climate patterns.

AUTOCORRELATION: The phenomenon in which the current value of a random time series variable is correlated with its value in a previous time period.

BIOMASS: The total quantity or weight of organic matter in a given area or volume.

CARBON: The 6th element in the periodic table. Popularly used as a shorthand term for carbon dioxide.

CMIP (Coupled Model Intercomparison Project): The Intergovernmental Panel on Climate Change's organized effort to compare many different global ocean-atmosphere climate models when they are all forced with the same emissions scenario(s). The two most recent were the CMIP5 and CMIP6 for, respectively, AR5 and AR6.

CLIMATE CHANGE: XXX

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CONUS: The contiguous 48 U.S. states.

DICE (Dynamic Integrated model of the Climate and Economy): An Integrated Assessment Model developed by William Nordhaus in 1993.

DISCOUNT RATE: The interest rate used to convert future costs and benefits into their present value. Varies inversely with present value so a high discount rate reduces the present value of future costs and benefits.

DRY ICE: Frozen carbon dioxide.

ECONOMETRICS: The branch of economics concerned with the use of statistical methods (chiefly multiple regression) to analyse economic systems.

ECS (Equilibrium Climate Sensitivity): The total amount of eventual global-average surface warming in response to a hypothetical doubling of the atmospheric CO₂ concentration compared to pre-Industrial levels.

EL NIÑO: The warm phase of the El Niño – Southern Oscillation, involving weaker upwelling of cold water off the South American coast and weaker Pacific trade winds.

EMISSIONS SCENARIO: An assumed scenario of future greenhouse gas emissions (or resulting atmospheric concentrations) based upon assumptions regarding global economic activity, the prevalence of fossil fuel use, and (sometimes) global carbon cycle model estimates of the rate of CO₂ uptake by land and ocean.

ENDANGERMENT FINDING (EF): The 2009 finding by the Administrator of the Environmental Protection Agency that emissions of well-mixed greenhouse gases (primarily CO₂) endanger human health and welfare.

ENSO: El Niño – Southern Oscillation, a prominent natural climate fluctuation involving year-to-year variations in the upwelling of cold water off the equatorial coast of South America and associated strengthening or weakening of trade winds across the Pacific Ocean.

FACE: (Free-Air CO₂ Enrichment) Large-scale open-air studies that expose plants to elevated CO₂ levels, simulating future climate scenarios.

FOSSIL FUEL: A natural hydrocarbon fuel such as coal, oil, or natural gas formed in the geological past from the remains of living organisms.

FUND (Framework for Uncertainty, Negotiation, and Distribution): An Integrated Assessment Model developed by Richard Tol in 1997.

GBR (Great Barrier Reef): The world's largest coral reef ecosystem located off the northeast coast of Australia.

GDP (Gross Domestic Product): Total market value of all the final goods and services produced within a country's border, usually over one year.

GHCN (Global Historical Climatology Network): A time-varying number of globally distributed weather stations making hourly, daily, or monthly measurements of precipitation, temperature and sometimes snowfall and snow depth.

GLOBAL GREENING: The satellite-observed increase in greenness in most vegetated land areas observed since the early 1980s.

GNP (Gross National Product): Total market value of all the final goods and services produced by the citizens of a country, including exports and imports, usually over one year.

GDP (Gross National Product): Total market value of all the domestically-produced final goods and services produced by the citizens of a country, usually over one year.

GREENHOUSE EFFECT (GHE): The tendency for any planetary atmosphere containing greenhouse gases to be warmer in the lowest layers than if those gases did not exist.

OUO – Official Use Only

GREENHOUSE GAS (GHG): An atmospheric gas that absorbs and emits infrared radiation, especially water vapor, CO₂, and methane.

HURST PHENOMENON: See long term persistence.

HYDROLOGY: The study of the movement of water, especially on land and in the atmosphere.

IAM (Integrated Assessment Model): A computerized tool that combines economics, climate science, and social sciences to provide quantitative relationships between human and Earth systems, helping to inform policy decisions.

IEA (International Energy Agency): A Paris-based intergovernmental organization, established in 1974, that provides policy recommendations, analysis and data on the global energy sector.

IPCC (Intergovernmental Panel on Climate Change): A panel of international experts and governmental representatives established by the United Nations and World Meteorological Organization in 1988 to provide governments at all levels with scientific information that they can use to develop climate policies.

IR (Infrared): Heat radiation that all solid objects and greenhouse gases emit by virtue of their temperature.

LA NIÑA: The cool phase of ENSO, involving stronger upwelling of cold water off the South American coast and stronger Pacific trade winds.

LONG TERM PERSISTENCE (LTP, or HURST PHENOMENON): A form of autocorrelation in which random fluctuations induce long term but not permanent changes in the mean value of a series, which shows up in some climate data series as cyclical or trending patterns on short time scales.

MAIZE: Corn.

MODEL: A computer code or collection of codes which quantify our knowledge of various processes and the interactions between those processes.

MORTALITY: Death or number of deaths, usually expressed for a specific population and period.

NCA (National Climate Assessment): A periodic report mandated by the Global Change Research Act of 1990 summarizing the impact of climate change on the United States. There have been five NCA reports: NCA1 (2000), NCA2 (2009), NCA3 (2014), NCA4 (2017-18), and NCA5 (2023).

OECD (Organization for Economic Cooperation and Development): A group of 38 member countries that have a democratic system of government and free economic systems.

PALEOCLIMATE: Climate of the distant past, before modern weather instruments were widely used (typically pre-1850), necessitating the use of “proxy” measurements such as tree rings, ice core, pollen records, etc.

PDI (Power Dissipation Index): A statistical estimate of the accumulated destructive potential of a tropical cyclone over time, calculated as a summation of the cube of the maximum sustained wind speeds.

PDO: Pacific Decadal Oscillation, a recurring ocean-atmosphere interaction centered over the Pacific basin associated with predominantly warm or cool patterns that influence the global climate.

PHOTOSYNTHESIS: The process by which plants grow, requiring carbon dioxide, water, and light at sufficiently warm temperatures (10 to 35 deg. C for optimal function).

PPM: Parts per million, a measure of the concentration of a substance in (for example) the atmosphere

PRE-INDUSTRIAL: The near-term historical period before which human greenhouse gas emissions were considered significant, usually assumed to be the mid-1700s.

RADIATIVE FEEDBACK: A change in the balance between absorbed solar radiation and emitted infrared (IR) radiation, referenced to the top of the atmosphere, caused by changes in surface temperature.

RADIATIVE FORCING: The change in the balance between absorbed solar radiation and emitted infrared (IR) radiation, referenced to the top of the atmosphere, caused by changes in greenhouse gases, anthropogenic aerosols, volcanoes, etc.

RCP (Representative Concentration Pathway) SCENARIOS: Different scenarios for future greenhouse gas emissions and their impact on the atmosphere, introduced in the IPCC's Fifth Assessment Report (AR5). RCP scenarios have a number label (e.g. RCP6.0) indicating how much radiative forcing they assume in 2100 relative to pre-Industrial times, in Watts per square meter. RCP4.5 and RCP6.0 are intermediate scenarios, whereas RCP8.5 is an extreme scenario with very large future emissions of greenhouse gases.

REGRESSION: A statistical method for selecting the coefficients of a linear equation to explain the behaviour of a dependent variable in terms of variations in one or more explanatory variables.

RICARDIAN ANALYSIS: A method used in environmental economics named after David Ricardo to estimate the economic impacts of climate change, particularly on agriculture, by making use of the hypothesis that expected changes in the return to future agricultural activity will be capitalized into current land values.

SCC (Social Cost of Carbon): An estimate of the net economic damages, measured in present-value dollars, caused by emitting one additional ton of CO₂ into the atmosphere.

SSP (Shared Socioeconomic Pathway) SCENARIOS: Different scenarios for future greenhouse gas emissions and their impact on the atmosphere, introduced in the IPCC's Sixth Assessment Report (AR6). Some of the SSP scenarios are analogous to the older RCP scenarios, for purposes of continuity and comparison to earlier climate model assessments.

TC: Tropical cyclone.

TMAX: The daily maximum surface air temperature.

TMIN: The daily minimum surface air temperature.

TOA: Top of atmosphere.

TOXICOLOGY: The study of the adverse effects of chemical substances on living organisms.

TREND: A coefficient, usually estimated using linear regression, representing the slope of a line of best fit through a time series of data, indicating any tendency for the series mean to move up or down over time.

TSI (Total Solar Irradiance): The measure of the total amount of incident solar radiation per unit area, including all wavelengths of electromagnetic radiation, that reaches the Earth's atmosphere. Typically shown in units of watts per square meter (W/m²).

UHI (Urban Heat Island): The tendency for inhabited areas to be warmer than their rural surroundings, due to the replacement of natural land and vegetation with roads, parking lots, buildings, and waste heat sources.

USHCN: The U.S. Historical Climatology Network, consisting of quality-controlled temperature and precipitation data from 1,218 surface weather stations.

METADATA FOR FIGURES AND TABLES

Figure 2.1 Screen shot from Figure 3 of Zhu et al. 2016 [HYPERLINK "<https://www.nature.com/articles/nclimate3004>"]

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Figure 2.2 Data replotted from Berner 2006 Figure 18, with annotation in red added by authors [HYPERLINK "<https://www.sciencedirect.com/science/article/pii/S0016703706002031>"]

Figure 2.3 Screen shot from Gerhart and Ward (2010) Figure 2 [HYPERLINK "<https://nph.onlinelibrary.wiley.com/doi/pdf/10.1111/j.1469-8137.2010.03441.x>"]

Figure 2.4 Screenshot from Copernicus Marine Services [HYPERLINK "https://data.marine.copernicus.eu/product/GLOBAL_OMI_HEALTH_carbon_ph_area_averaged/description"]

Figure 2.5 Screen shot from AIMS (2023) Figures 2-4 [HYPERLINK "https://www.aims.gov.au/sites/default/files/2022-08/AIMS_LTMP_Report_on%20GBR_coral_status_2021_2022_040822F3.pdf"]

Figure 3.1.1 Screen shot of AR6 WGI Ch2 Fig 10

Figure 3.1.2 Screen shot of AR6 WGI Ch7 Fig 7-6

Figure 3.1.3 Author created figure. CO2 data source: [HYPERLINK "<https://gml.noaa.gov/ccgg/trends/index.html>"]

Figure 3.2.1 Screen shot from Hausfather et al. (2019) Figure S4 [HYPERLINK "<https://doi.org/10.1029/2019GL085378>"]

Figure 3.2.2 Author created figure. Data source: Freidlingstein et al. (2024) [HYPERLINK "<https://essd.copernicus.org/preprints/essd-2024-519>"]
Data source: [HYPERLINK "<https://globalcarbonbudget.org/download/1442/>"]

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Figure 3.2.3 Author-created using OLS trends, Data source: [HYPERLINK "<https://globalcarbonbudget.org/download/1442/>"]

Figure 3.2.4 Author-created using OLS trends, Data source: [HYPERLINK "<https://globalcarbonbudget.org/download/1442/>"]

Figure 4.1 Screen shot from Scafetta (2021) Figure 1 [HYPERLINK "<https://doi.org/10.3390/cli9110161>"]

Figure 5.1 Data source: [HYPERLINK "<https://climexp.knmi.nl/start.cgi>"]. Author-created figure. One ensemble member per model, yearly average temperatures are computed from the global monthly raw temperatures; 'range' is the warmest model temperature minus the coolest model temperature in each year; standard deviation computed across all 33 models' raw temperatures in each year.

Figure 5.2 Screen shot from Scafetta (2023) Figure 2 [HYPERLINK "<https://doi.org/10.1007/s00382-022-06493-w>"]

Figure 5.3 Author created figure. Data source: [HYPERLINK "<https://climexp.knmi.nl/start.cgi>"]. One SSP370 ensemble member per model. Linear OLS trends computed from global monthly temperature anomalies relative to 1981-2010.

Figure 5.4 Author created figure, based on Figure 3 of McKittrick and Christy (2020) [HYPERLINK "<https://doi.org/10.1029/2020EA001281>"]. Updated using same methods and data extended from 2014 to 2024, see McKittrick and Christy (2025) in chapter references.

Figure 5.6 Screen shot of IPCC AR5 Figure 10.SM1, with author annotations

Figure 5.7 Author created figure following Christy and McNider (2017) [HYPERLINK "<https://doi.org/10.1007/s13143-017-0070-z>"] with updated data available at [HYPERLINK "https://www.nsstc.uah.edu/data/cmip6/ERA5_REANALYSES/"] and [HYPERLINK "https://www.nsstc.uah.edu/data/cmip6/JRA3Q_REANALYSES/"] compiled here [HYPERLINK "https://www.nsstc.uah.edu/data/cmip6/CWG25_Fig_5.7_Table_250627.xlsx"] [HYPERLINK "<https://doi.org/10.1007/s13143-017-0070-z>"]

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Figure 5.8 Screen shot from [HYPERLINK "https://climate.rutgers.edu/snowcover/chart_seasonal.php?ui_set=nhland&ui_season=1"] (accessed May 27, 2025)

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Figure 5.9 Screen shot from Rugenstein and Hakuba (2023) Figure 1 [HYPERLINK "<https://doi.org/10.1029/2022GL101802>"]

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Figure 5.10 Author created figure. Data source [HYPERLINK "<https://climexp.knmi.nl/start.cgi>"]. OLS linear trends. One SSP370 ensemble member per model, averaged surface air temperatures for June through August over 31N to 49N latitude, 84W to 102W longitude. Observed temperature data for same months from NOAA/NCEI at <https://www.ncei.noaa.gov/access/monitoring/climate-at-a-glance/statewide/time-series> for the 12 Corn Belt states averaged together.

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Figure 6.1.1 Author replot of Figure 10 from Koutsoyiannis (2013) –~~F~~ [HYPERLINK "<http://dx.doi.org/10.1080/02626667.2013.804626>"]

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Figure 6.2.1 Screen shot of figure from Maue (2025) [HYPERLINK "<https://climatlas.com/tropical/>"]

Figure 6.2.2 Author created figure. Data from National Hurricane Center (2024) [HYPERLINK "<https://www.nhc.noaa.gov/climo/>"]

Figure 6.2.3 Author created figure. Data from NOAA HRD [HYPERLINK "https://www.aoml.noaa.gov/hrd/hurdat/All_U.S._Hurricanes.html"] \t "_new"]

Table 6.2.1 Author created table. Data from NOAA HRD [HYPERLINK "https://www.aoml.noaa.gov/hrd/hurdat/most_intense.html"] \t "_new"]

Figure 6.3.1 Screen shot from NCA4 Figure 6.4

Figure 6.3.2 Author created figure. Data source: [HYPERLINK "https://www.nsstc.uah.edu/data/ushcn_jrc/"]

Figure 6.3.3 Author created figure. Data source: [HYPERLINK "https://www.nsstc.uah.edu/data/ushcn_jrc/"]

Figure 6.3.4 Author created figure. Data source: [HYPERLINK "https://www.nsstc.uah.edu/data/ushcn_jrc/"]

Figure 6.3.5 Author created figure Data source: [HYPERLINK "https://www.nsstc.uah.edu/data/ushcn_jrc/"]

Figure 6.3.6 Author created figure. Data source: [HYPERLINK "https://www.nsstc.uah.edu/data/ushcn_jrc/"]

Figure 6.3.7 Screen shot from [HYPERLINK "<https://www.globalchange.gov/indicators/heat-waves>"]

Box tables. Author created table. Data from McKittrick and Christy (2025)

Figure 6.4.1 Author created figure. Data from McKittrick and Christy (2025)

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Figure 6.4.3 Author created figure. Data from McKittrick and Christy (2025)

Figure 6.4.4 Author created figure. Data from McKittrick and Christy (2025)

Figure 6.4.5 Author created figure. Data from McKittrick and Christy (2025)

Figure 6.5.1 Author created figure. Data source: [HYPERLINK "https://www.spc.noaa.gov/wcm/data/1950-2024_actual_tornadoes.csv"]

Figure 6.7.1 Author created figure. Data source: [HYPERLINK "<https://www.ncei.noaa.gov/access/monitoring/uspa/wet-dry/0>"] ~~accessed June 16, 2025. OLS trend line added.~~

~~accessed June 16, 2025. OLS trend line added.~~

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Figure 6.8.1 Screen shot from Lizundia-Loiola *et al.* (2021) Figure 12.

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Figure 6.8.2 Screenshot from Marlon *et al.* (2012) Figure 2 panel C. [HYPERLINK "<https://www.pnas.org/doi/pdf/10.1073/pnas.1112839109>"]

Figure 6.8.3 Author created Figure. Data 1926 to 2016: [HYPERLINK "https://web.archive.org/web/20200212033452/https://www.nifc.gov/fireInfo/fireInfo_stats_totalFires.html"]. Post-2017 [HYPERLINK "<https://www.nifc.gov/fire-information/statistics/wildfires>"] Accessed June 16, 2025.

Figure 7.1 Screenshot from [HYPERLINK "<https://tidesandcurrents.noaa.gov/sltrends/>"]

Table 7.1 Author created table. From Section 6.2 of [HYPERLINK "<https://judithcurry.com/wp-content/uploads/2018/11/special-report-sea-level-rise-3.pdf>"]

Figure 7.2 Screenshot from [HYPERLINK
"https://tidesandcurrents.noaa.gov/sltrends/sltrends_station.shtml?id=9414290"] (downloaded 4/22/25)

Figure 7.3 Screenshot from
[HYPERLINK "https://tidesandcurrents.noaa.gov/sltrends/sltrends_station.shtml?id=8771450"]
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Figure 7.4 Screenshot from
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Figure 7.5 Screenshot from
[HYPERLINK "https://tidesandcurrents.noaa.gov/sltrends/sltrends_station.shtml?id=8518750"]
(downloaded 4/22/25)

Figure 7.6 Author created figure. Data from [HYPERLINK
"https://tidesandcurrents.noaa.gov/sltrends/sltrends_station.shtml?id=8518750"]

Figure 8.1 Screenshot from
[HYPERLINK "https://crudata.uea.ac.uk/cru/data/temperature/HadCRUT5.0Analysis.pdf"]

Figure 8.2 Screenshot from Hansen and Karecha (2025) Figure 1
[HYPERLINK "https://www.columbia.edu/~jeh1/mailings/2025/CloudFeedback.13May2025.pdf"]

Table 8.1 Screenshot of column 1 of Table 12.12 IPCC AR6 Working Group I report

Figure 9.1 Screenshot From Taylor and Schlenker (2021) Figure 1
[HYPERLINK "https://www.nber.org/papers/w29320"]

Figure 9.2 Screenshot from McKittrick (2025) Figure 1
[HYPERLINK "https://www.nature.com/articles/s41598-025-90254-2"]

Figure 10.1 Screenshot from Pielke (2024) [HYPERLINK "https://www.nature.com/articles/s44304-024-00011-0"] Figure 3.

Figure 10.2 Screenshot from Gasparini et al. (2015) Figure 2
[HYPERLINK "http://dx.doi.org/10.1016/S0140-6736(14)62114-0"]

Figure 11.1 Screen shot from Pielke Jr (2023)
[HYPERLINK "https://rogerpielkejr.substack.com/p/global-disaster-losses1990-2023"]

Figure 11.2 Screenshot from CEA-OMB (2023) [HYPERLINK
"https://bidenwhitehouse.archives.gov/wp-content/uploads/2023/03/CEA-OMB-White-Paper.pdf"] Figure
1.

ABOUT THE AUTHORS

John Christy, Ph.D. is the Distinguished Professor of Atmospheric and Earth Sciences and Alabama's State Climatologist at the University of Alabama in Huntsville. He has a B.A. (Mathematics) from CSU Fresno and an M.S. and Ph.D. (Atmospheric Sciences) from the University of Illinois in Urbana-Champaign. He is a Fellow of the American Meteorological Society and recipient of The NASA Medal for Exceptional Scientific Achievement for constructing, with Dr. Roy Spencer, the first global atmospheric temperature dataset from polar orbiting satellites. He served as Lead Author of the 3rd IPCC Assessment Report (2001), he has coauthored the National Academy of Science expert report on Climate Observations (1995) and on Surface Temperature Reconstructions (2006), and he has served on the NOAA Climate Change Science Program, NASA's Earth Science Subcommittee, the National Research Council Space Studies Board and the EPA Science Advisory Board. In addition, Dr. Christy has given testimony in 20 Congressional Hearings.

Judith Curry, Ph.D. is Professor Emerita at the Georgia Institute of Technology, where she served as Chair of Earth and Atmospheric Sciences for 13 years. She is President and co-founder of Climate Forecast Applications Network (CFAN). Curry has a PhD in Geophysical Sciences from the University of Chicago. She has authored or coauthored 192 peer-reviewed papers in atmospheric and climate sciences, two textbooks, and most recently the book *Climate Uncertainty and Risk*. Curry is a Fellow of the American Meteorological Society, the American Association for the Advancement of Science, American Geophysical Union, and a Member of the American Academy of Sciences and Letters. She has served on several Science Steering Committees of the World Climate Research Program and also the DOE Biological & Environmental Research Advisory Committee, Earth Science Subcommittee of the NASA Advisory Council, and the NRC Space Studies Board and Climate Research Committee. Dr. Curry has given testimony in 13 Congressional Hearings. ~~is Professor Emerita at the Georgia Institute of Technology, where she served as Chair of Earth and Atmospheric Sciences for 13 years. She is President and co-founder of Climate Forecast Applications Network (CFAN). Curry has a PhD in Geophysical Sciences from the University of Chicago. She has authored or coauthored 192 peer-reviewed papers in atmospheric and climate sciences, two textbooks, and most recently the book *Climate Uncertainty and Risk*. Curry is a Fellow of the American Meteorological Society, the American Association for the Advancement of Science, American Geophysical Union, and a Member of the American Academy of Sciences and Letters.~~

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The Honorable Steven E. Koonin, Ph.D. is the Edward Teller Senior Fellow at Stanford's Hoover Institution. He has served as Under Secretary for Science at the Department of Energy (2009-2011), as Chief Scientist for British Petroleum (2004-2009), and as a professor at Caltech (1975 – 2004, the last nine years as Vice President and Provost). Koonin is a member of the National Academy of Sciences and the JASON group of government advisors, as well as a Governor of Lawrence Livermore National Laboratory, having served in similar capacities for the Los Alamos, Sandia, Brookhaven, and Argonne National Laboratories. Other prior roles include a University Professor at New York University, a non-resident Senior Fellow at the American Enterprise Institute, and a Trustee of the Institute for Defense Analyses. Koonin has a BS in Physics from Caltech and a PhD in Theoretical Physics from Massachusetts Institute of Technology. He authored the 2021 bestseller *Unsettled: What Climate Science Tells Us, What It Doesn't, and Why It Matters*, the 1985 textbook *Computational Physics*, and some 200 peer-reviewed papers in physics and astrophysics, scientific computation, energy technology and policy, and climate science.

Ross McKittrick, Ph.D., is a Professor of Environmental Economics at the University of Guelph in Ontario, Canada. He holds a BA in economics from Queen's University, and an MA and Ph.D. in economics

from the University of British Columbia. He has published widely in both economics and physical science journals on topics related to climate change, pollution and public policy. His book *Economic Analysis of Environmental Policy* was published by the University of Toronto Press in 2010. His background in applied statistics has led him to author or coauthor peer-reviewed publications across a wide range of topics in the physical sciences including paleoclimate reconstruction, malaria transmission, surface temperature measurement, climate attribution methodology and climate model evaluation. Professor McKittrick has made many invited academic presentations around the world, was an invited contributor to the 2006 National Academy of Sciences report on Surface Temperature Reconstructions and has testified before the U.S. Congress and committees of the Canadian House of Commons and Senate.

Roy W. Spencer, Ph.D., is a Principal Research Scientist at the University of Alabama in Huntsville. He holds a BS in Atmospheric and Oceanic Science from the University of Michigan, and MS and PhD in Meteorology from the University of Wisconsin -Madison. Spencer's published research has focused on monitoring of weather processes and climate change from Earth-orbiting satellites, as well as using simple physical models for diagnosing climate sensitivity and climate feedbacks. As a NASA Senior Scientist for Climate Studies he served as the U.S. Science Team Leader for the Advanced Microwave Scanning Radiometer (AMSR-E) flying on the Aqua satellite, providing our most accurate global measurements of sea surface temperatures, sea ice, and several other features of the climate system. Along with Dr. John Christy, Spencer developed the first satellite-based technique for global temperature monitoring, for which they received NASA's Exceptional Scientific Achievement Medal and the American Meteorological Society's Special Award.

From: Ross McKittrick <ross.mckittrick@gmail.com>
Sent: Friday, June 27, 2025 9:49 PM
To: Judith Curry; Roy W. Spencer; John Christy; Steven Koonin; Travis Fisher; Josh Loucks; Cohen, Seth
Subject: June 27 version of CWG report
Attachments: DoE.Report.ERRATA.FIXED.June27.docx

All errata fixed including Fig 5.7, all metadata listed.

Have a nice weekend everyone.

Â

From: Dr. Ronan Connolly <ronan@ceres-science.com>
Sent: Monday, August 18, 2025 4:17 PM
To: Judith Curry
Cc: Soon, Ph.D, Willie; Steve Koonin; John Christy; Roy Spencer; Ross McKittrick
Subject: Re: Requesting your help with comments on the DOE report

Hi Judy,

Yes, that works for me! Keep me posted & we can arrange a Zoom next week if necessary.

Good luck & best wishes,
Ronan

*Dr. Ronan Connolly
Center for Environmental Research and Earth Sciences (CERES)*

*CERES website: <https://www.ceres-science.com/>
Personal website: <https://ronanconnollyscience.com/>
Publications: [ResearchGate](#), [GoogleScholar](#)*

On Monday, August 18th, 2025 at 4:15 PM, Judith Curry <curry.judith@gmail.com> wrote:

Hi Ronan, thanks very much for your response. I suggest that I go through the relevant comments in more detail and attempt a response, then we can discuss the outstanding ones that i didn't feel comfortable addressing. I would say sometime next week for call, if that would work for you?

On Mon, Aug 18, 2025 at 1:11 PM Dr. Ronan Connolly <ronan@ceres-science.com> wrote:
Hi Judy et al.,

First, thanks for the kind words! Aline and I just got back this weekend from travels after the wedding.

Willie and I both independently skimmed through the Carbon Brief comments - both the ones you specifically highlighted and the others. Our impression is that most of the so-called "False" statements, other than a few minor and trivial typos, e.g., referring to Scafetta (2023) as Scafetta et al. (2023), are not actually "False" but rather different perspectives. Almost all of the so-called "Misleading" statements are not misleading - they are just differences of opinion, and this was flagged throughout your DOE report, including the Executive Summary.

What you have done with your DOE report is an important service and the Carbon Brief "fact-check" is itself littered with "Misleading" and/or "False" statements. So many that it is hard to know where to begin.

Willie & I would like to help you in formulating your responses to the comments for which we can be of assistance. But, because it is such a long "[Gish gallop](#)" and many of the comments are probably easy for you to respond to without our feedback, maybe the best thing would be if we arrange a Zoom to discuss it. You could then hone in on any specific comments we could help you with. Would that work?

If so, when would suit? I'm busy today, tomorrow and Wednesday, but could Zoom towards the end of the week, or next week if that suits. Willie says he is also available then.

In the meantime, in terms of the specific comments related to your discussion of our papers, most of the longest comments come from Dr. Theodosios Chatzistergos (he/him). He appears to have a personal vendetta against us. He's a post-doc at the Max Planck Institute (MPI) who also did his PhD there under the joint supervision of Profs. Natalie Krivova and Sami Solanki. Krivova & Solanki developed the "SATIRE" series of TSI proxy-based reconstructions that make up 50% of the Matthes et al. (2017) TSI reconstruction. He seems to have a very strong personal attachment to this reconstruction and others that are similar, e.g., the NRL proxy-based TSI reconstruction developed by Dr. Judith Lean et al., that makes up the other 50% of the Matthes reconstruction.

Theodosios' *modus operandi* seems to be to (1) firstly ignore any criticism, then if necessary (2) straw-man it, and finally (3) lie and make up a false narrative about it. Last year, he published a sloppy pseudoscientific article applying all three steps to Hoyt & Schatten (1993), one of the few TSI reconstructions to explicitly try to model the multi-decadal TSI variability without using sunspot numbers (SSN), since SSN is highly problematic for studying inter-cycle changes during the solar minima because SSN = 0 during every solar minimum. Both SATIRE and the NRL reconstructions are very heavily weighted on SSN and show almost no TSI variability between solar minima.

A month after he published his paper that he claims "thoroughly discredited" Hoyt & Schatten (1993), we published this response on the CERES website: <https://www.ceres-science.com/post/response-to-chatzistergos-2024>

I then spent a tedious amount of time trying to engage with him on X, but it was largely unproductive as he doesn't seem to have any genuine interest in the science. If you are interested, I posted two X articles about my failed attempts to have any genuine interactions with him:

1. <https://x.com/1RonanConnolly/status/1789055206472159326>
2. <https://x.com/1RonanConnolly/status/1900970653144031571>

We plan on writing a formal peer-reviewed response to Chatzistergos (2024) at some stage. But, to be honest, his paper is mostly just an extended strawman argument to promote his assertion that there has been almost no inter-cycle variability in TSI during solar minima. So, it isn't a particularly interesting paper from a scientific perspective, and hence it hasn't been a high priority work for us so far.

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I hope that is of some help. Let us know if you would like to go ahead with a group Zoom, and if so, when would suit best.

Best wishes,
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*Dr. Ronan Connolly
Center for Environmental Research and Earth Sciences (CERES)*

*CERES website: <https://www.ceres-science.com/>
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On Thursday, August 14th, 2025 at 7:31 PM, Judith Curry <curry.judith@gmail.com> wrote:

Hello Ronan and Willie,

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From: Judith Curry <curry.judith@gmail.com>
Sent: Monday, August 18, 2025 4:15 PM
To: Dr. Ronan Connolly
Cc: Soon, Ph.D, Willie; Steve Koonin; John Christy; Roy Spencer; Ross McKittrick
Subject: Re: Requesting your help with comments on the DOE report

Hi Ronan, thanks very much for your response. I suggest that I go through the relevant comments in more detail and attempt a response, then we can discuss the outstanding ones that i didn't feel comfortable addressing. I would say sometime next week for call, if that would work for you?

On Mon, Aug 18, 2025 at 1:11 PM Dr. Ronan Connolly <ronan@ceres-science.com> wrote:
Hi Judy et al.,

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Sent: Monday, August 18, 2025 4:12 PM
To: Judith Curry
Cc: Soon, Ph.D, Willie; Steve Koonin; John Christy; Roy Spencer; Ross McKittrick
Subject: Re: Requesting your help with comments on the DOE report

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From: Judith Curry [curry.judith@gmail.com]
Sent: 8/14/2025 11:31:34 PM
To: Dr. Ronan Connolly [ronan@ceres-science.com]; Soon, Ph.D, Willie [romeosoon@gmail.com]; Steve Koonin [steven.koonin@gmail.com]; John Christy [climateman60@gmail.com]; Roy Spencer [roywspencer@hotmail.com]; Ross McKittrick [ross.mckittrick@gmail.com]
Subject: Requesting your help with comments on the DOE report
Attachments: Connolly:soon.docx

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MISLEADING

The IPCC has downplayed the role of the sun in climate change, but there are plausible solar irradiance reconstructions that imply it contributed to recent warming.

[[HYPERLINK "https://www.mps.mpg.de/solar-physics/staff/27569" \h](https://www.mps.mpg.de/solar-physics/staff/27569)],
astrophysicist, Max Planck Institute for Solar System Research

This is a vague and potentially misleading statement. The IPCC AR6 adopted the solar radiative forcing values recommended by CMIP6, while AR5 used those from CMIP5. In both cases, the TSI [total solar irradiance] reconstructions selected were the most advanced and extensively scrutinised available at the time (Matthes et al. (2017)). The notion that there exist "plausible" solar irradiance reconstructions implying a solar contribution to recent warming is misleading, since it leaves it vague as to how much it contributed. The most scientifically robust irradiance reconstructions indicate that this contribution has been relatively small in recent decades. The wording of this statement appears to implicitly reference the work of Connolly et al. (2021) and (2023), although only the former is cited in the DoE report. Connolly et al. (2021) argued that a significant portion, or even most, of recent global warming could be attributed to solar variability. However, their approach involved cherry-picking and balancing 16 TSI reconstructions: eight with low long-term variability and eight with high variability. To create this balance, they included outdated or superseded models while omitting many low-variability reconstructions.

The high-variability series used by Connolly et al. (2021):

- Hoyt and Schatten (1993) has been thoroughly discredited (see Chatzistergos (2024)). It includes arbitrary manual adjustments and even fabricated data, with some values seemingly copied from another solar index.

- Lean et al. (1995) is an early version of the NRL [Naval Research Laboratory] model that has since been superseded by multiple updated versions, most recently NRLTSI2/NNL (Coddington et al. (2016) and (2019)).

- Bard et al. (2000) used a simplified linear conversion from cosmogenic isotope production (e.g., ^{10}Be) to TSI, without accounting for nonlinear effects such as geomagnetic modulation, atmospheric transport processes, or climate-driven deposition variations. These omissions have been addressed in more physically realistic models like SATIRE-M [Spectral And Total Irradiance REconstructions] (Wu et al. (2018)), which show significantly lower long-term variability.

Furthermore, Delaygue and Bard (2011) revised the original Bard et al. (2000) reconstruction which was exhibiting low long-term variability, yet this update was ignored by Connolly et al. (2021).

- Shapiro et al. (2011), an older high-variability model, was effectively replaced by Egorova et al. (2018), which itself has been criticised by Yeo et al. (2020) for exaggerating long-term TSI changes. - Egorova et al. (2018) was included four times in the Connolly et al. (2021) analysis. Including Shapiro et al. (2011), a predecessor of Egorova's model, the same model family was effectively used five times, artificially inflating the weight of high-variability reconstructions.

Crucially, in all these high-variability models, the long-term TSI trends were not derived from observational

constraints but were imposed as assumptions. Modern revisions of the NRL model (Lean (2018) and Coddington et al. (2019)) and the Hoyt and Schatten model (Chatzistergos (2024)) have significantly reduced these assumed trends to align with observational evidence. Therefore, treating these outdated, superseded, or methodologically flawed reconstructions as equally probable alternatives to modern, validated TSI models is scientifically unjustified. It creates a false equivalence that misrepresents the current state of solar irradiance research. Connolly et al. (2023), although not cited in the DoE report, extended the analysis to 27 TSI series. However, the same methodological issues persist. Many of the added reconstructions are either outdated or redundant versions of older models.

Notably, the highest solar attribution results again stem from Hoyt and Schatten (1993) and Bard et al. (2000), both of which have been superseded and shown to be unreliable. Finally, the attribution method used by Connolly et al. (2021) was directly critiqued by Richardson and Benestad (2022), who identified substantial flaws in their regression methodology and treatment of uncertainty. Even with the analysis presented in Connolly et al. (2021) and (2023) when the outdated, superseded and implausible TSI series are removed, the result is that the solar contribution to global warming is significantly smaller than the anthropogenic one.

1.1 Components of radiative forcing and their history

Page 12

MISLEADING

The IPCC assesses the change in the radiative forcing by the sun to be negligible, based on their preference for data reconstructions that imply minimal solar change since pre-industrial times.

[[HYPERLINK "https://www.mps.mpg.de/solar-physics/staff/27568"](https://www.mps.mpg.de/solar-physics/staff/27568) \h],
astrophysicist, Max Planck Institute for Solar System Research

The selection of TSI reconstructions for use in CMIP6 and IPCC AR6 was based on the scientific robustness and performance of the models, rather than the magnitude of TSI variability they imply (Matthes et al. (2017)). The two reconstructions adopted were SATIRE and NRLTSI (now referred to as NNLTSI). These models were chosen because they represent the most advanced TSI reconstructions currently available. SATIRE is a semi-empirical, physics-based model, while NNLTSI is a proxy-based regression model. Both have been extensively validated and demonstrate excellent agreement with direct satellite measurements of TSI, making them the most reliable choices for climate modeling to date.

3.1 Components of radiative forcing and their history

Page 12

MISLEADING

But Connolly et al. (2021) reviewed 16 different Total Solar Irradiance (TSI) reconstructions in the literature covering the years 1600-2000; the reconstructions vary from almost no change in TSI to a relatively large upward trend. Those authors note that the variation in TSI reconstructions combined with variations in surface temperature reconstructions allows for inferences consistent with either no or most 20th century warming being attributable to the sun.

Dr Theodosius Chatzistergos, astrophysicist, Max Planck Institute for Solar System Research

The approach by Connolly et al. (2021) involved cherry-picking and balancing 16 TSI reconstructions: eight with low long-term variability and eight with high variability. To create this balance, they included outdated or superseded models while omitting many low-variability reconstructions.

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3.1 Components of radiative forcing and their history

Page 13

MISLEADING

A particularly thorny issue is the gap in TSI data between 1989 and 1991 due to a delay in the launch of a monitor following the Space Shuttle Challenger

” [A gap in TSI measurements between the ACRIM-1 and ACRIM-2 satellite monitoring experiments] as a “particularly thorny” issue. While it does contribute to uncertainty in the long-term trend of direct TSI measurements, a growing body of evidence challenges the notion of a TSI increase during the ACRIM-gap, evidence which the DoE report entirely overlooks. Notable examples include Amdur & Huybers (2023), Chatzistergos et al. (2025) and Krivova et al. (2009).

Quote given to Carbon Brief

3.1 Components of radiative forcing and their history

Page 13

FALSE

Connolly et al. (2021) found that the IPCC’s consensus statements on solar forcing were formulated prematurely through the suppression of dissenting scientific opinions.

[[HYPERLINK "https://www.mps.mpg.de/solar-physics/staff/27559"](https://www.mps.mpg.de/solar-physics/staff/27559) \h],
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This is yet another statement in the DoE report that is vague. It is not clear what they refer to with consensus, is this about the long-term reconstructions, is it just about the ACRIM-gap that this paragraph discusses? In any case, it appears as a general statement which as such has to be labelled as false. The claim that dissenting scientific opinions are being suppressed is unfounded.

Scientific consensus evolves through rigorous evaluation, not censorship. When significant methodological issues are identified, such as the arbitrary adjustments and fabricated data in the Hoyt and Schatten (1993) TSI reconstruction (Chatzistergos (2024)), it is scientifically appropriate to disregard such models as implausible. This is not suppression of dissenting voices, but the enforcement of standards for methodological soundness.

Another example is the Bard et al. (2000) TSI reconstruction, which has since been superseded by more advanced models. Bard et al. (2000) relied on a simplistic linear scaling of cosmogenic isotope production to estimate TSI variability, without accounting for the non-linear influence of geomagnetic field modulation on isotope production. This omission leads to inaccurate estimates of solar variability. Later reconstructions, such as those based on the SATIRE-M framework (e.g., Wu et al. (2018)), incorporate these critical non-linearities and have been shown to produce more consistent results with observational constraints. Albeit, uncertainties remain and more work is being invested in improving these reconstructions too. Furthermore, we have advanced physics-based irradiance reconstruction models, e.g. SATIRE-3D (Yeo et al. (2017) and (2020)), which have been used to set robust constraints on the magnitude of plausible TSI variations. As a result, irradiance reconstructions that exceed this constraint are considered less plausible than those that remain within it. Disregarding outdated or methodologically flawed reconstructions is a hallmark of scientific integrity, not suppression. The continued use of discredited models undermines robust scientific discourse and misrepresents the state of knowledge. A reminder here that the study by Connolly et al. (2021) cherry-picked 16 TSI reconstructions, deliberately balancing the sample by including eight with small long-term trends and eight with pronounced trends. The high-variation models they used are Hoyt and Schatten (1993), Lean (1995), Bard et al. (2000), Shapiro et al. (2011) and Egorova et al. (2018; used 4 different versions).

Besides the issues with Hoyt and Schatten (1993) and Bard et al. (2000) discussed above, Lean (1995) is an outdated precursor to the NRL (now NNL) model and is no longer considered plausible. Shapiro et al. (2011) and Egorova et al. (2018) are different versions of the same model both exceeding the Yeo et al. 2020 constraint.

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MISLEADING

AR5 concluded that the best estimate of radiative forcing due to Total Solar Irradiance (TSI) changes over the period 1750–2011 was very small (0.05 W/m², Myrhe et al. (2014)).

[[HYPERLINK "https://www.mps.mpg.de/solar-physics/staff/27562"](https://www.mps.mpg.de/solar-physics/staff/27562) \h],
astrophysicist, Max Planck Institute for Solar System Research

This is an example of imprecise wording that could be misleading. In AR5, the solar radiative forcing difference between the 1745 and 2008 activity minima was estimated using TSI reconstructions from Wang et al. (2005), Steinhilber et al. (2009) and Krivova et al. (2010). These studies yielded a range of -0.02 to 0.071 Watts per metre squared (W/m²) for the difference (based on seven-year running means, with the caveat that the -0.02 W/m² value comes from Steinhilber et al. (2009), which has a five-year resolution and uses 1965 as the minimum). Based on these estimates, AR5 adopted a range of 0–0.1 W/m² with a central estimate of 0.05 W/m², as explicitly stated in Table 8.SM.4 of the report. However, AR6 defined solar forcing in a different way to reflect the difference between full solar cycles rather than solar minima, as noted in chapter 7 of the WG1 report. This change in methodology is not acknowledged in the DoE report, which may lead to misunderstandings or incorrect comparisons between AR5 and AR6 values.

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MISLEADING

AR6 acknowledges substantially higher values and a much larger range of estimates of changes in TSI over the last several centuries, stating that the TSI between the Maunder Minimum (1645–1715) and the second half of the 20th century increased by 0.7–2.7 W/m², a range that includes both low and high variability TSI data sets (Gulev (2021)).

[[HYPERLINK "https://www.mps.mpg.de/solar-physics/staff/27561"](https://www.mps.mpg.de/solar-physics/staff/27561) \h],
astrophysicist, Max Planck Institute for Solar System Research

The sentence is vague (in particular as to what they refer with "higher values") and potentially conflates two related but distinct concepts: radiative forcing and TSI differences, which were both mentioned but not clearly distinguished. Considering that the previous sentence discusses radiative forcing, one can assume

that with "higher values" they refer to radiative forcing too, which would make the statement false. This is easily understood by comparing the statements in AR6 and AR5 about the range of solar radiative forcing.

The AR6 WG1 report (page 958) states:

"In contrast to AR5, the solar effective radiative forcing (ERF) in this assessment uses full solar cycles rather than solar minima. The pre-industrial TSI is defined as the mean from all complete solar cycles from the start of the 14C SATIRE-M proxy record in 6755 BCE to 1744 CE. The mean TSI from solar cycle 24 (2009–2019) is adopted as the assessment period for 2019. The best estimate solar ERF is assessed to be 0.01 W/m², using the 14C reconstruction from SATIRE-M, with a likely range of -0.06 to +0.08 W/m² (medium confidence). The uncertainty range is adopted from the evaluation of Lockwood and Ball (2020), who performed a Monte Carlo analysis of solar activity from the Maunder Minimum to 2019 using several datasets, resulting in an ERF range of -0.12 to +0.15 W/m². The Lockwood and Ball (2020) full uncertainty range is halved since the period of reduced solar activity in the Maunder

Minimum had ended by 1750 (medium confidence)." By comparison, the AR5 WGI report (page 689) states:

"The best estimate from our assessment of the most reliable TSI reconstruction gives a seven-year running mean radiative forcing (RF) between the minima of 1745 and 2008 of 0.05 W/m². Our assessment of the range of RF from TSI changes is 0.0 to 0.10 W/m²."

Thus, the solar radiative forcing estimates in AR5 and AR6 are broadly consistent, with AR6's best estimate slightly lower. If the statement instead refers to TSI differences, this would also be misleading. The AR6 WGI (page 297) lists a TSI difference between the Maunder Minimum and the second half of the 20th century ranging from 0.7 to 2.7 W/m². The lower bound likely corresponds to the SATIRE-T reconstruction, while the upper bound comes from the Yeo et al. (2020) constraint on the dimmest possible state of the sun. It is important to note that Yeo et al. (2020) does not reconstruct TSI during the Maunder Minimum but rather provides a theoretical constraint on the minimum plausible solar irradiance. This constraint effectively excludes TSI reconstructions with large amplitude variability, such as Egorova et al. (2018).

Almost all current TSI reconstructions indicate significantly smaller long-term TSI trends (Chatzistergos et al. (2023)). These include the SATIRE and NRL (now NNL) models recommended for CMIP6 and AR6.

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Page 84

FALSE

The IPCC has only minimally discussed solar influences on global and regional climate.

[[HYPERLINK](http://www.personal.reading.ac.uk/~ym901336/index.html)

"<http://www.personal.reading.ac.uk/~ym901336/index.html>" \h],
professor of space environment physics and president of the Royal Astronomical Society,
University of Reading

Contrary to this statement, there is much discussion in the last three IPCC assessment reports.

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MISLEADING

There is substantial evidence for high solar activity in the second half of the 20th century (starting in 1959) and extending into the 1990s, before a decline in the early 21st century; this period is often termed the “Modern Maximum” (Chatzistergos et al. (2023); Solanki et al. (2004); Usoskin et al. (2007)). However, some scientists have concluded that it is not possible to be confident of any multi-decadal trend in TSI (Schmutz (2021)).

[[HYPERLINK "https://www.mps.mpg.de/person/27558/2169"](https://www.mps.mpg.de/person/27558/2169) \h], astrophysicist, Max Planck Institute for Solar System Research

“Although the sentence in which they cite us is factually accurate in isolation, the surrounding text creates a misleading impression. More importantly, our review paper, which they cited, explains why their subsequent paragraph has misleading and wrong statements.”

Quote from [[HYPERLINK "https://bsky.app/profile/damagedonegr.bsky.social"](https://bsky.app/profile/damagedonegr.bsky.social) \h]

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MISLEADING

However, the recommended forcing dataset for the CMIP6 climate model simulations used in AR6 for attribution studies averages two data sets with low solar variability (Matthes (2017)).

[[HYPERLINK "https://www.mps.mpg.de/solar-physics/staff/27558"](https://www.mps.mpg.de/solar-physics/staff/27558) \h], astrophysicist, Max Planck Institute for Solar System Research

interpreted as an actual historical TSI difference between the Maunder Minimum and the present, but rather as a physical upper bound on the maximum plausible difference between the sun's dimmest possible state and its current irradiance. Importantly, the Maunder Minimum is unlikely to represent this dimmest state and modern TSI reconstructions suggest a substantially smaller long-term trend than what a 2.7 W/m² Maunder-to-present difference would imply. This upper limit, derived from Yeo et al. (2020), effectively excludes high-variability TSI reconstructions that exceed this threshold. For this reason, the CMIP6 and IPCC AR6 selected two TSI reconstructions, SATIRE (Yeo et al. (2014) and Wu et al. (2018)) and NRLTSI (now referred to as NNLTSI, Coddington et al. (2016), not based on their implied variability magnitude, but because they represent the most advanced, scientifically robust, and rigorously validated models currently available (Matthes et al. (2017)). SATIRE is a semi-empirical,

physics-based model, whereas NRLTSI is a proxy-based regression model. Both have undergone extensive scrutiny and show excellent agreement with direct satellite measurements of TSI.

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MISLEADING

While AR6 shows a substantially greater solar impact than does AR5, the overall impact of solar forcing on the climate was still assessed to be small compared to anthropogenic forcing.

[[HYPERLINK "https://www.mps.mpg.de/solar-physics/staff/27558"](https://www.mps.mpg.de/solar-physics/staff/27558)
\\h], astrophysicist, Max Planck Institute for Solar System Research

The statement is vague and unclear in what it means by “AR6 shows a substantially greater solar impact than AR5”, especially since the same sentence acknowledges that solar forcing was assessed to be small compared to anthropogenic forcing, implying a correspondingly small solar impact. This apparent contradiction likely stems from confusion on the part of the DoE authors regarding the distinction between radiative forcing and total solar irradiance (TSI) differences as presented in AR5 and AR6, as discussed in previous comments.

Quote given to Carbon Brief

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MISLEADING

However, the impact of solar variations on the climate is uncertain and subject to substantial debate (Lockwood (2012); Connolly et al. (2021)) - something that is not evident in the IPCC assessment reports.

[[HYPERLINK "https://www.mps.mpg.de/solar-physics/staff/27559"](https://www.mps.mpg.de/solar-physics/staff/27559)
\\h], astrophysicist, Max Planck Institute for Solar System Research

AR6 acknowledges the high-variability reconstruction by Egorova et al. (2018) and references studies by Yeo et al. (2020) and Lockwood & Ball (2020), discussing the inherent uncertainty in estimating the secular trend in solar irradiance variations. The earlier study by Lockwood (2012) is not included because its findings are superseded by the more recent Lockwood & Ball (2020). Mike Lockwood was actually one of the contributors to AR6. Thus, IPCC AR6 did adequately discuss the existing uncertainty in irradiance modelling and, thus, its effects on Earth's climate. However, what the authors of the DoE report seem to mean with this sentence is that AR6 does not discuss the Connolly et al. (2021) study. It is worth repeating that the Connolly et al. (2021) paper gave a misleading presentation of the literature. In particular, Connolly et al. (2021) cherry-picked 16 TSI reconstructions, deliberately balancing the sample by including eight with small long-term trends and eight with pronounced trends. However, their inclusion of several outdated or discredited models within the high-variability group is misleading, particularly given the DoE chapter summary's claim that all selected reconstructions are plausible (Chatzistergos et al. (2023) and Chatzistergos (2024)).

As noted in previous comments, the issues with Connolly et al. (2021) undermine the credibility of their results. While there is legitimate uncertainty about the exact magnitude of long-term solar irradiance trends, Connolly et al. (2021) exaggerated the solar impact by cherry-picking outdated and superseded series that display implausibly large TSI variations.

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MISLEADING

There are several rival composite TSI datasets that disagree as to whether TSI increased or decreased during the period 1986-96.

[[HYPERLINK "https://www.mps.mpg.de/solar-physics/staff/27559"](https://www.mps.mpg.de/solar-physics/staff/27559)

\h], astrophysicist, Max Planck Institute for Solar System Research

It is also important to note that the effect of ACRIM-gap in the trends of the different TSI composites is generally small (Chatzistergos et al. (2023) and Kopp (2025)). While the issue with ACRIM does contribute to the uncertainty in the long-term trend of direct TSI measurements, a growing body of evidence challenges the notion of a TSI increase during the ACRIM-gap, evidence which the DoE report entirely overlooks. Notable examples include Amdur & Huybers (2023), Chatzistergos et al. (2025) and Krivova et al. (2009). By having given such emphasis on the ACRIM-gap the DoE report gives the misleading impression that there can be significant increase in TSI, which could account for the global warming over the same period. Thus, this statement can be misleading.

MISLEADING

Further, the satellite record of TSI is used to calibrate proxy models that infer past solar variations from sunspots and cosmogenic isotope measurements (Velasco Herrera et al. (2015)).

[[HYPERLINK "https://www.mps.mpg.de/solar-physics/staff/27560"](https://www.mps.mpg.de/solar-physics/staff/27560)
\h], astrophysicist, Max Planck Institute for Solar System Research

There are several issues with this sentence, particularly concerning the choice of the sole reference, which may mislead readers into believing that existing total solar irradiance (TSI) reconstructions depend heavily on direct TSI measurements for determining long-term trends. This is generally incorrect. Moreover, the phrasing gives the impression that only proxy models exist, overlooking the important class of physics-based semi-empirical models

1)Missing relevant literature: The statement omits key papers on irradiance reconstruction models (e.g., Coddington et al. (2016); Wu et al. (2018); and Chatzistergos et al. (2024)) and authoritative reviews (e.g., Chatzistergos et al. (2023) and (2024); and Solanki et al. (2013)) that provide a more accurate and comprehensive perspective.comprehensive perspective.

2)Choice of reference: The DoE cites only Velasco Herrera et al. (2015) to support the claim, which is problematic. That work is not an irradiance reconstruction model but a machine learning extrapolation inherently dependent on the chosen TSI reference series. Given the chaotic nature of solar activity, such extrapolations from limited data lack scientific merit (see Petrovay (2020)). Moreover, the predictions from Velasco Herrera et al. (2015) for 2015–25 are already contradicted by actual direct TSI measurements obtained since.

1) Proxy models: These reconstruct irradiance variations by scaling or regressing solar activity indices against reference TSI data. While the choice of reference series can influence results depending on the regression method, the effect is not always big. For example, Chatzistergos et al. (2020) demonstrated that the choice of TSI reference has negligible impact on minimum-to- minimum trends in their regression model. Similarly, Figure 7c in Chatzistergos et al. (2024) shows only a minor effect of different TSI references on their century-long TSI reconstruction.

2) We have not only proxy models but also physics-based semi-empirical models. Models such as SATIRE and SRPM do rely on tuning some free parameters by comparison to direct TSI measurements. However, this tuning does not significantly affect the long-term trends, as shown in multiple studies (e.g., Chatzistergos et al. (2021)). Notably, Chatzistergos et al. (2025) explicitly states about their irradiance reconstruction with SATIRE-S that "the trend in the updated SATIRE-S TSI composite is independent of measured TSI".

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MISLEADING

There is substantial evidence for high solar activity in the second half of the 20th century (starting in 1959) and extending into the 1990s, before a decline in the early 21st century; this period is often termed the "Modern Maximum." (Chatzistergos et al. (2023); Solanki et al. (2004); Usoskin et al. (2007))

[[HYPERLINK "https://www.mps.mpg.de/solar-physics/staff/27566"](https://www.mps.mpg.de/solar-physics/staff/27566) \h], astrophysicist, Max Planck Institute for Solar System Research

The citation of Chatzistergos et al. (2023) in support of the statement about the "Grand Modern Maximum" of solar activity is somewhat misleading. First, the cited paper is a review focused specifically on solar irradiance modeling and long-term trends in irradiance reconstructions. The Grand Modern Maximum is mentioned only briefly in the introduction and is not a subject of analysis in the paper. Moreover, the concept of the Grand Modern Maximum is primarily based on direct sunspot observations, as discussed in works such as Chatzistergos et al. (2017) and Usoskin et al. (2016), which is clearly stated in Chatzistergos et al. (2023), but omitted in the DoE report. The other two studies cited in the DoE, Solanki et al. (2004) and Usoskin et al. (2007), inferred this period in sunspot number series reconstructed from cosmogenic isotope data.

However, these data do not cover the full extent of the modern maximum, and the connection between isotope-based and direct sunspot records remains uncertain. Recent reconstructions like Wu et al. (2018; where both Sami K. Solanki and Ilya G. Usoskin are co-authors) continue to support the existence of a Grand Modern Maximum, but suggest it was less pronounced than earlier assumed.

I presume the authors do not include this study because it was used to reconstruct irradiance variations and returned a rather small secular trend, which would conflict with the claim they want to make here. It is important to recognise that elevated sunspot activity during the second

half of the 20th century does not, on its own, determine how much solar irradiance increased, as information on faculae is also essential. Irradiance variations are driven not only by sunspots but also by faculae and network magnetic features.

Unfortunately, direct facular observations only extend back to 1892, and their use prior to the 1970s has been rather limited. Consequently, reconstructing past irradiance variations requires assumptions about the relationship between sunspots and faculae, which introduces uncertainty into estimates of the secular trend. However, recent efforts to extract facular information from Ca II K observations (e.g., Chatzistergos et al. (2024)) and to extend irradiance reconstructions back to 1892 indicate only a minimal long-term trend over the 20th century, consistent with state-of-the-art reconstructions such as SATIRE. As mentioned above also updated reconstructions with cosmogenic isotope data (Wu et al. (2018)) suggest a rather small long-term trend. None of this McKittrick (2025) re-examined the Moore et al. database and found that, while it claimed

to cover 1,722 studies, only half the entries (N=862) had complete context is provided in the DoE report, resulting in a potentially oversimplified or overstated implication that a large secular trend is more likely, despite the balance of current evidence.

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MISLEADING

However, some scientists have concluded that it is not possible to be confident of any multi-decadal trend in TSI (Schmutz, 2021).

[[HYPERLINK "https://www.mps.mpg.de/solar-physics/staff/27567"](https://www.mps.mpg.de/solar-physics/staff/27567)
\h], astrophysicist, Max Planck Institute for Solar System Research

This statement is misleading because it refers to section 3 of Schmutz (2021), which focuses on direct measurements of solar irradiance only since 1978, whereas the DoE report in the previous sentence was discussing the grand modern maximum and thus longer-term trends beyond the period covered by direct TSI measurements. Furthermore, the uncertainty in determining the long-term trend, both from direct measurements and modeled reconstructions, is widely acknowledged in the scientific literature and is not limited to a small group of scientists. Nevertheless, all current evidence and advances consistently indicate a relatively weak secular trend in total solar irradiance (TSI) variations.

Quote given to Carbon Brief

MISLEADING

This uncertainty causes some reconstructions of TSI from 1750 to have low variability (implying a very low impact of solar variations on global mean surface temperature) whereas datasets with high TSI variability can explain more than 70% of the temperature variability since pre-industrial times (Scafetta (2013); Stefani (2021)).

[[HYPERLINK "https://www.mps.mpg.de/solar-physics/staff/27558"](https://www.mps.mpg.de/solar-physics/staff/27558) \h], astrophysicist, Max Planck Institute for Solar System Research

Neither of the two papers cited for the claim that "datasets with high TSI variability can explain more than 70% of the temperature variability since pre-industrial times". Stefani (2021) did not even use any TSI series; instead, he performed a linear regression between the geomagnetic aa-index and sea surface temperature. Scafetta (2013, p27) states: "Figure 15B compares the Central England Temperature (CET) record and the TSI model by Hoyt and Schatten plus the ACRIM TSI record; an overall good correlation is observed since 1700, which suggests that the major observed climatic oscillations are solar induced and that the sun explains about 50-60% of the warming observed since 1900."

This raises two major concerns. First, the statement in the report is incorrect since neither Scafetta (2013) nor Stefani (2021) demonstrate that TSI can account for "more than 70% of the temperature variability since preindustrial times". Second, the conclusion by Scafetta (2013) relies on the Hoyt and Schatten (1993) TSI model after connecting it to the ACRIM TSI composite. The Hoyt and Schatten 1993 TSI model has been shown to be highly unrealistic and is now widely discredited (Chatzistergos (2024)). As detailed in Chatzistergos (2024), the Hoyt and Schatten model includes multiple arbitrary adjustments and fabricated data for one index spanning over a decade, which appear to have been copied from another index. Critically, the model's high variability was not a result of the model but was imposed by the authors. When updated underlying indices and direct TSI composites (e.g., PMOD, Montillet et al. (2022), ACRIM) are considered, it becomes clear that Hoyt and Schatten (1993) exaggerated TSI variation amplitudes by roughly a factor of five (Chatzistergos (2024)). The discrepancy is even greater when compared to the ACRIM TSI composite, which Hoyt and Schatten's model is incompatible with. This seriously undermines the reliability of conclusions drawn from the Hoyt and Schatten model for attributing recent climate change to solar variability.

Therefore, the two given references do not corroborate the claim of the DoE report. This statement likely originates from Connolly et al. (2023), but the same criticisms previously discussed regarding Connolly et al. (2021) apply here as well. Connolly et al. (2021) and (2023) cherry-picked TSI reconstructions to amplify several outdated or discredited models with high secular trends. As noted in previous comments, these issues undermine the credibility of their results. While there is legitimate uncertainty about the exact magnitude of

long-term solar irradiance trends, Connolly et al. (2021) and (2023) exaggerated the solar impact by cherry-picking outdated and superseded series that display implausibly large TSI variations.

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MISLEADING

The choice of TSI satellite record used in an analysis can therefore substantially influence how much climate change is attributed to human versus natural forcings.

[[HYPERLINK "https://www.mps.mpg.de/solar-physics/staff/27559"](https://www.mps.mpg.de/solar-physics/staff/27559) \h], astrophysicist, Max Planck Institute for Solar System Research

This statement is misleading and confusing. First, the choice of TSI satellite record does not substantially influence the estimated solar contribution to climate change; rather, it is the choice of modelled TSI series, which was the topic of the previous sentence, that can have an impact. Second, as explained above, the high-variability TSI reconstructions used by Connolly et al. (2021) and (2023) to attribute most warming to the Sun are outdated, superseded, and scientifically implausible. While there is indeed uncertainty regarding the long-term trend in solar irradiance, which naturally affects the precise estimate of solar-driven global warming, all current evidence indicates this contribution is relatively small. The sentence in the DoE report, however, dramatically exaggerates this uncertainty and its implications.

However, it is also possible that the authors of the DoE report intended to suggest that the choice of TSI satellite records influences the reconstruction outcomes and thus the estimated solar contribution to global warming. This interpretation would also be misleading, as explained earlier, but reiterated here:

- 1) While the choice of reference TSI series can affect proxy model results depending on

the regression approach, this effect is often small. For example, Chatzistergos et al. (2020) demonstrated that the choice of TSI reference has a negligible impact on minimum-to-minimum trends in their regression model. Similarly, Figure 7c in Chatzistergos et al. (2024) shows only minor differences when using various TSI references in their century-long reconstruction.

2) This effect is typically negligible in physics-based semi-empirical models. Various studies have confirmed the minimal impact on SATIRE reconstructions when changing the reference TSI series (e.g., Chatzistergos et al. 2021). Notably, for the latest version of SATIRE-S, Chatzistergos et al. (2025) explicitly state that "the trend in the updated SATIRE-S TSI composite is independent of measured TSI".

3) As mentioned earlier, the DoE authors cited Velasco Herrera et al. (2015) instead of irradiance reconstruction models, which may have caused the confusion. Velasco Herrera et al. (2015) presented a machine learning extrapolation that inherently depends on the chosen TSI reference series. Given the chaotic nature of solar activity, such limited-data extrapolations lack scientific robustness (see Petrovay (2020)). Moreover, Velasco Herrera et al. 2015 predictions for 2015-25 have already been contradicted by direct TSI measurements collected since.

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FALSE

Scafetta et al. (2023) suggests that ~80% of solar influence on climate might stem from non-TSI mechanisms.

[[HYPERLINK "https://www.mps.mpg.de/solar-physics/staff/27559"](https://www.mps.mpg.de/solar-physics/staff/27559) \h], astrophysicist, Max Planck Institute for Solar System Research

The reference is incorrect, there is no Scafetta et al. (2023); this should be Scafetta (2023). Although Scafetta (2023) indeed makes this claim, it is yet another such claim of attributing most global warming to the sun mainly based on the use of the discredited Hoyt and Schatten (1993) TSI series (Chatzistergos (2024)). Scafetta (2023) bases his analysis on a combination of three TSI series: Hoyt and Schatten (1993), Egorova et al. (2018) and the one recommended by CMIP6. Since the Hoyt and Schatten (1993) TSI series was not updated prior to Chatzistergos (2024), Scafetta extended it by linking it to the ACRIM TSI composite. Chatzistergos (2024) demonstrated that the high variability imposed by Hoyt and Schatten (1993) is inconsistent with direct TSI measurements and that a variability magnitude approximately five times smaller is required.

Additionally, Chatzistergos (2024) showed that the Hoyt and Schatten (1993) model conflicts particularly with the ACRIM composite by exhibiting opposite trends during the so-called ACRIM-gap, making Scafetta's extension of Hoyt and Schatten (1993) with ACRIM invalid. Thus, with Egorova et al. (2018) exceeding the Yeo et al. (2020) physical constraint, and considering the multiple methodological issues identified with the Hoyt and Schatten (1993) series (Chatzistergos (2024)), the resulting conclusions by Scafetta (2023) are unlikely. Furthermore, Scafetta himself acknowledged that even when allowing for solar effects beyond radiative forcing, the solar influence remains minimal with the CMIP6 TSI forcing. Therefore, the claim that solar influence accounts for more than 80% of warming is unsupported.

Quote given to Carbon Brief

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MISLEADING

There are numerous candidate processes, including solar ultraviolet changes; energetic particle precipitation; atmospheric-electric-field effect on cloud cover; cloud changes produced by solar-modulated galactic cosmic rays; large relative changes in the magnetic field; and the strength of the solar wind. Such solar indirect effects are not included in climate models, although indirect methods of estimating their impacts suggest they are significant.

[[HYPERLINK "https://www.mps.mpg.de/solar-physics/staff/27560"](https://www.mps.mpg.de/solar-physics/staff/27560)
\h], astrophysicist, Max Planck Institute for Solar System Research

It is exaggerated and incorrect to claim that the impacts of such effects are significant. Many of these effects are described vaguely in the DoE report, making their implications unclear (for example, "large relative changes in the magnetic field") without specifying what that entails. Additionally, the influence of cosmic rays on cloud formation has been shown to be negligible, as demonstrated by the CLOUD experiment at CERN (Pierce (2017)). While some effects on Earth's atmosphere from other processes have been studied (e.g., Mironova et al. (2015) and Sinnhuber & Funke (2020)), their effects are generally considered to be small.

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MISLEADING

However, the recommended forcing dataset for the CMIP6 climate model simulations used in AR6 for attribution studies averages two data sets with low solar variability (Matthes, (2017)).

[HYPERLINK

"<http://www.personal.reading.ac.uk/~ym901336/index.html>" \h],
professor of space environment physics and president of the Royal Astronomical Society,
University of Reading

The statement is true, but very misleading as it is made to sound that larger TSI-drift papers were ignored. There is only one very high TSI drift reconstruction and it is very much an outlier – other reconstructions show much smaller drift. There is also analysis of recent solar cycles that shows that the low TSI change reconstructions are correct.

8.3 Attribution of global warming

MISLEADING

However, the impact of solar variations on the climate is uncertain and subject to substantial debate (Lockwood (2012); Connolly et al. (2021)) – something that is not evident in the IPCC assessment reports.

[HYPERLINK

"<http://www.personal.reading.ac.uk/~ym901336/index.html>" \h],
professor of space environment physics and president of the Royal Astronomical Society,
University of Reading

This is highly misleading as it conflates regional and global climate. The cited references were discussing regional climates where there is more uncertainty and some (limited) solar influence through downward propagation of stratospheric responses, especially in winter. The global climate is not open to such uncertainty. The cooling (as opposed to warming) of the global stratosphere shows conclusively that the effect of solar change on the global climate is minimal compared to effects of changing solar irradiance.

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MISLEADING

However, the recommended forcing dataset for the CMIP6 climate model simulations used in AR6 for attribution studies averages two data sets with low solar variability solar variability (Matthes (2017)).

[[HYPERLINK "https://www.geomar.de/en/kmatthes"](https://www.geomar.de/en/kmatthes) \h], director,
GEOMAR Helmholtz Centre for Ocean Research Kiel

It is correct that the long-term trend in TSI is rather on the low variability side, but it is completely ignored that other more extreme trends (high variability trends) are completely unrealistic. We only provide an extreme Maunder Minimum case as a sensitivity experiment.

8.3 Attribution of global warming

Page 85

MISLEADING

While AR6 shows a substantially greater solar impact than does AR5, the overall impact of solar forcing on the climate was still assessed to be small compared to anthropogenic forcing. However, the impact of solar variations on the climate is uncertain and subject to substantial debate (Lockwood (2012); Connolly et al. (2021) – something that is not evident in the IPCC assessment reports. The variations of TSI over time remains a challenging problem. Since 1978, there have been direct measurements of TSI from satellites. However, the data exhibits non-negligible inconsistencies, and interpreting any multi-decadal trends in TSI requires comparisons of observations from overlapping satellites. There are several rival composite TSI datasets that disagree as to

whether TSI increased or decreased during the period 1986-96 (the ACRIM gap; see Chapter 4). Further, the satellite record of TSI is used to calibrate proxy models that infer past solar variations from sunspots and cosmogenic isotope measurements (Velasco Herrera et al. (2015)). There is substantial evidence for high solar activity in

the second half of the 20th century (starting in 1959) and extending into the 1990s, before a decline in the early 21st century; this period is often termed the “Modern Maximum”. (Chatzistergos et al. (2023); Solanki et al. (2004); Usoskin et al. (2007)). However, some scientists have concluded that it is not possible to be confident of any multi-decadal trend in TSI (Schmutz (2021)). This uncertainty causes some reconstructions of TSI from 1750 to have low variability (implying a very low impact of solar variations on global mean surface temperature) whereas datasets with high TSI variability can explain more than 70% of the temperature variability since pre-industrial times (Scafetta (2013); Stefani (2021)). The choice of TSI satellite record used in an analysis can therefore substantially influence how much climate change is attributed to human versus natural forcings.

[[HYPERLINK "https://www.geomar.de/en/kmatthes"](https://www.geomar.de/en/kmatthes) \h], director,
GEOMAR Helmholtz Centre for Ocean Research Kiel

In Matthes et al. (2017), we conclude the following two points (just as an example of mis-citation):

- 1) A new and lower TSI value is recommended: the contemporary solar-cycle average is now $1,361.0 \pm 0.5 \text{ W/m}^2$ (Prša et al. (2016)).
- 2) Over the last three solar cycles in the satellite era, there is a slight negative TSI trend in the CMIP6 dataset. A recent reconstruction of the TSI, with a proper estimation of its uncertainties, suggests that this downward trend between the solar minima of 1986 and 2009 is not statistically significant

(Dudok de Wit et al. (2017)). The TSI trend leads to an estimated radiative forcing on a global scale of -0.04 W/m^2 , which is small in comparison with other forcings over this period.

Therefore the sentence in the DoE report stating that the “choice of TSI satellite record used in an analysis can therefore substantially influence how much climate change is attributed to human versus natural forcings” is completely wrong and ignores the thorough science. The radiative forcing to this natural forcing – even if we would have chosen a different TSI dataset – is small (-0.04 W/m^2).

The next completely wrong statement is that “such solar indirect effects are not included in climate models”. In Matthes et al. (2017), we provide a comprehensive dataset including TSI and solar spectral irradiance data as well as solar particle forcing for the first time. We also provide an ozone dataset with the indirect effect included. Many of the CMIP6 models do include spectral solar irradiance as well as ozone changes, so the indirect effect and some even the particle effects. I am really shocked about these wrong statements. The IPCC is based on sound scientific evidence and this DoE report is kicking the evidence with feet.

Misinterpreting and mis-citing or not at all citing important work (such as the important parts of Matthes et al. (2017) where we provide sound scientific evidence and discuss the details) – this is just horrible. By taking out only parts of the story and "simplifying it", it is becoming simply wrong. This is not sound science."

From: Steven Koonin <koonin@stanford.edu>
Sent: Friday, August 15, 2025 5:37 PM
To: Ross McKittrick; Judith Curry
Cc: Roy Spencer; John Christy; Ison, Jeremy T.
Subject: RE: revisions

There are very few comments in parts of the text that I've been responsible for • But, of course, happy to be a second pair of eyes on whatever responses people generate.

Dr. Steven E. Koonin
Edward Teller Senior Fellow
Hoover Institution/Stanford University
koonin@stanford.edu

From: Ross McKittrick <ross.mckittrick@gmail.com>
Sent: Friday, August 15, 2025 4:35 PM
To: Judith Curry <curry.judith@gmail.com>
Cc: Steven Koonin <koonin@stanford.edu>; Roy Spencer <roy.spencer@nsstc.uah.edu>; John Christy <climateman60@gmail.com>; Ison, Jeremy T. <jeremy.ison@hq.doe.gov>
Subject: Re: revisions

OK, though I think you need a bigger monitor.
I can get started on mine later this month. I'm expecting there will be a lot of overlap across the various tranches.

On Fri, Aug 15, 2025 at 4:25 PM Judith Curry <curry.judith@gmail.com> wrote:

Mid Sept through Oct is bad for me. I think best for me to make edits in response to the comments as they come in, so there is a draft with my comments. I think the google hangouts process worked fine for the edits, travelling anywhere takes up alot of time

On Fri, Aug 15, 2025 at 1:18 PM Ross McKittrick <ross.mckittrick@gmail.com> wrote:

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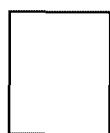
Judy does your schedule ease up in October? I think we could aim to begin assembling a revised version around then but likely not sooner. We'll certainly need video meetings but might benefit from gathering in person for a day or two late in the process. Maybe the DoE would be willing to pay to bring us together somewhere with a conference room so we can go through it line by line without having to squint at tiny screens for 8 hours.

On Fri, Aug 15, 2025 at 3:00 PM Judith Curry <curry.judith@gmail.com> wrote:

Anyone have thoughts on protocol for revising the CWG report? Simplest is for each of us to work from the final report and make our edits, then we can combine at some point. I need to start working on mine now, since 2nd half of Sept will be awful with the hurricanes. At the same time, will make notes on any reviews what changes have been made

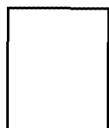
Re Carbon Brief solar comments, i went through these in detail and I can handle them (ronan is presumably out of commission for a bit). Will add some references and change some wordings, but I think the original ideas hold up ok

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Judith Curry, President
CFAN - Climate Forecast Applications Network
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curry.judith@cfanclimate.com | +1.404.803.2012
<http://www.cfanclimate.net>

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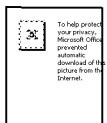
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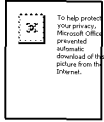
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<http://www.cfanclimate.net>



To help protect
your privacy,
Outlook prevented
automatic
download of this
picture from the
Internet.

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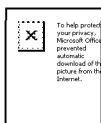
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From: Judith Curry <curry.judith@gmail.com>
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curry.judith@cfanclimate.com | +1.404.803.2012
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From: Judith Curry <curry.judith@gmail.com>
Sent: Monday, August 4, 2025 10:01 AM
To: John Christy
Cc: Roy Spencer; Ross McKittrick; Steve Koonin; Travis Fisher; Charles Park; Joshua Loucks; Seth Cohen; Audrey Barrios
Subject: comment portal

DOE folks, the comment portal now has 20 comments, but they can't be viewed (7 since friday). Can this be sped up so people can view the comments (i've gotten a number of email queries about this). Thanks



Judith Curry, President
CFAN - Climate Forecast Applications Network
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curry.judith@cfanclimate.com | +1.404.803.2012
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From: Judith Curry [curry.judith@gmail.com]
Sent: 8/20/2025 1:02:42 PM
To: Ross McKittrick [ross.mckittrick@gmail.com]
Subject: Fwd: FW: [Tropical-storms] DOE Report – Discussions on Tipping Points, Bifurcation, and Multistability
Attachments: Pages 123 - 125 TippingPointsHG_Emissions_on_the_US_Climate 123.pdf; Pages 123 - 125 TippingPointsHG_Emissions_on_the_US_Climate 124.pdf; Pages 123 - 125 TippingPointsHG_Emissions_on_the_US_Climate 125.pdf

FYI. As far as I can tell, i would only change the word "external force" to "external influence", although "forcing" in this context is common in the climate world

----- Forwarded message -----

From: **Montgomery, Michael (Mike) (CIV)** <mtmontgo@nps.edu>
Date: Tue, Aug 19, 2025 at 4:39 PM
Subject: FW: [Tropical-storms] DOE Report – Discussions on Tipping Points, Bifurcation, and Multistability
To: Judith Curry <curry.judith@gmail.com>, Steven Koonin <steven.koonin@gmail.com>, Richard Lindzen <rlindzen@mit.edu>, William Happer <happer@princeton.edu>, tim_dunkerton <tim_dunkerton@protonmail.com>, Joe Biello <biello@math.ucdavis.edu>

Subject: Re: [Tropical-storms] DOE Report – Discussions on Tipping Points, Bifurcation, and Multi-stability

Hi, Judy!

Following your recent reply that the current DOE text is pretty good, I decided to study it a bit. Please consider my comments as constructive and not as negative attacks.

Attached is the written text from the current DOE Report on tipping points. In one interpretation of the term, the report accepts the tipping point idea from simple nonlinear dynamical models (low-dimensional dynamical systems thinking, i.e.)

The first meaning of term “tipping point” is associated with forcing of an *inherently stable system* (my emphasis). In this context, the abrupt transition requires a sufficiently *strong external force*. The specific DOE text (on pg. 123) reads as follows:

“Such discontinuities are ubiquitous in nature and require an external force. Whether the force needs to be large relative to the size of the system depends on the underlying stability of the system.”

However, in science "Force" has a specific meaning ("a push or a pull") with units of Newton. How can a force be compared to the (volumetric?) size of a system? This makes no sense. Sorry. Newton, Landau and Feynman would be turning over in their graves! :-)

The second meaning of term "tipping point" is associated with an *inherently unstable system* and a "bifurcation" in a nonlinear dynamical system. The text reads as follows:

"Many systems have been observed to have more than one equilibrium point and can move between them with minimal or no external influence. For example, a weather system might have two different equilibrium states: one calm and one with a tornado. A transition from one to another can happen either with no external force or with a minuscule change, such as a flap of the proverbial butterfly's wings (Shen *et al.* 2014). The term "tipping point" is sometimes used to mean a bifurcation of this type and implies instability inherent in the system itself, which is not necessarily dependent on outside forces. It depends, instead, on parameters of the system taking values that support the emergence of bifurcations (Crawford, 1991)."

"The two different concepts imply different research questions.

[This text is pretty good - I concur.]

"Models have been developed that imply the second type is a possibility. Kypke *et al.* (2020) presented a simple climate model in which the GHG concentration is one of the parameters that controls the emergence of bifurcations of the Arctic climate to one with both cold and warm equilibria. If sufficiently high GHG forcing combined with a sufficiently high rate of ocean heat transport are imposed a bifurcation becomes possible. More generally GCMs have been observed to contain bifurcations and multiple equilibria (Brunetti and Ragon, 2023)."

I will have to study this GCM study that allegedly reports bifurcations. But notwithstanding this specific study, Palmer and Stevens 2019 PNAS article suggests that we are not yet capable of identifying these purported bifurcations due to climate model inaccuracies (they show *inter alia* large

spread among CMIP-5 models - with the largest spread among models greater than the small warming signal, itself!).

The rest of the DOE text seems to accept the second definition without any mention of the issues I raised in my recent email and issues that have been revealed (courtesy Joe Biello) since the 1990's (re Lorenz equations and others) about such very low-dimensional models not being derivable asymptotically from the equations of motion in a consistent truncation with a suitable small parameter and distinguished limit, etc., and in a valid part of the parameter space.

Perhaps the text is adequate for its purpose (certainly not the text about Force being compared to the size of system!), but I personally would not want to go with this or present this to an audience without some discussion about low versus high dimensional complex systems and the known limitations of these low dimensional systems and the complexity of identifying tipping points in high dimensional turbulent systems.

In meantime. I will try and look at the above mentioned GCM study, but I fear examining it since GCMS are junk science IMO.

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From: Judith Curry <curry.judith@gmail.com>

Sent: Monday, August 18, 2025 8:23:04 AM

To: Montgomery, Michael (Mike) (CIV) <mtmontgo@nps.edu>

Subject: Re: [Tropical-storms] DOE Report – Discussions on Tipping Points, Bifurcation, and Multistability

NPS WARNING: *external sender* verify before acting.

Actually, i think the tipping points text in CH 11 of the DOE report is pretty good

On Mon, Aug 18, 2025 at 12:29 AM Montgomery, Michael (Mike) (CIV) <mtmontgo@nps.edu> wrote:

Judy et al:

I would be very cautious about over referencing these ideas developed from LOW-DIMENSIONAL Dynamical Systems to the INFINITE-DIMENSIONAL CLIMATE DYNAMICS problem.

Tipping points controlled by one (or a few) non-dimensional parameters and which determine the abrupt change of the system are much harder to find (or non-existent) in infinite dimensional complex systems of nonlinear partial differential equations describing the *turbulent atmosphere-ocean fluid dynamical system*. The only known examples of “tipping” points in the fluid dynamical context describe:

- i) the transition to a turbulent state (via Reynolds number or Taylor number in the rotating Taylor-Couette problem);
- or
- ii) transition from a thermally conductive solution to a convective (overturning or plume-dominated regime) in the Rayleigh-Benard problem.

These are “tipping points” near the transition between laminar and turbulent flow. Once the flow has already transitioned to a turbulent one (such as our atmosphere-ocean system in its present state), the tipping points and “control parameters” are much more indefinite (if they exist at all) and entail disparate time scales. Anyone who promotes without critical analysis (observational support and fluid dynamical reasoning covering coherent structures of the turbulent flow with multiple time scales, etc.) the premise that CO2 concentration is the primary control knob for the infinite-dimensional turbulent radiative-thermodynamic-mechanical system should be viewed very skeptically IMO until they can address the foregoing points directly.

(N.B. Two of our colleagues, Michael E. McIntyre and Ray Pierrehumbert have invoked a number of tipping points (> ~6 by my count) involving ice dynamics, methane, CO2, water vapor (aka weather fuel), turbulent ocean upwelling from the deep ocean to ventilate the carbon lying on sea floors See McIntyre’s recent textbook entitled *Science, Music and Mathematics: The Deepest Connections. Postlude. The amplifier metaphor for climate and endnotes*. I have studied this chapter and related endnotes this summer and do not accept what I view as the faulty premise that *the atmosphere-ocean system is intrinsically unstable to “external inputs”*. The early faint sun paradox and Lindzen’s Iris (and its generalization that Tim Dunkerton, Joe Biello & I have discussed) are arguably the best counter examples and **not ever mentioned** in McIntyre’s 42 page chapter or endnotes.

From: Tropical-storms <tropical-storms-bounces@lists.tstorms.org> on behalf of Bo-Wen Shen via Tropical-storms <tropical-storms@lists.tstorms.org>

Date: Saturday, August 16, 2025 at 7:07 AM

To: Judith Curry <curry.judith@gmail.com>

Cc: Tropical-Storms <Tropical-storms@lists.tstorms.org>

Subject: Re: [Tropical-storms] DOE Report – Discussions on Tipping Points, Bifurcation, and Multistability

NPS WARNING: *external sender* verify before acting.

Dear Judith:

Thanks very much.

On the cover page, we provided the following:

The purposes are threefold, as suggested by the DOE website:

1. Editorial – Correct an in-text miscitation in Chapter 11, where *Shen et al.* is incorrectly cited as “2014” instead of “2023.”
2. Reference – Ensure that the corrected in-text citation is properly linked to the already correct *Shen et al. (2023)* entry in the reference list.
3. Technical – Provide additional scientific context, in the form of further reading, regarding the relationship between tipping points, bifurcation points, and multistability. As our 2023 study is specifically referenced in the report’s discussion of tipping points, these clarifications and supplemental materials are intended to ensure accuracy and to enrich the scientific understanding presented.

A final version will be submitted early next week. Thank you!

Best,

-Bowen

On Sat, Aug 16, 2025 at 6:16 AM Judith Curry <curry.judith@gmail.com> wrote:

Hello Bo-Wen, it would be very helpful if you submitted a specific comment/suggestion to the DOE comment portal. Thank you

<https://www.federalregister.gov/documents/2025/08/01/2025-14519/notice-of-availability-a-critical-review-of-impacts-of-greenhouse-gas-emissions-on-the-us-climate>

On Sat, Aug 16, 2025 at 3:56 AM Stoffelen, Ad (KNMI) via Tropical-storms <tropical-storms@lists.tstorms.org> wrote:

Dear Bowen,

It is much appreciated that you try and provide further scientific understanding of the consequences of Greenhouse gas emissions. On the other hand, we already have ten thousands of peer-reviewed manuscripts providing understanding and through peer-reviewing by experts a sound view of these consequences. How would this single simplified study help the scientists that were involved in the DOE report?

No doubt your manuscript can be a pearl among all the others, while I believe it is not scientific understanding that is the prime topic of the DOE report.

Cheers,

Ad

From: Tropical-storms <tropical-storms-bounces@lists.tstorms.org> **On Behalf Of** Bo-Wen Shen via Tropical-storms

Sent: zaterdag 16 augustus 2025 00:21

To: Tropical-Storms <Tropical-storms@lists.tstorms.org>

Subject: [Tropical-storms] DOE Report – Discussions on Tipping Points, Bifurcation, and Multistability

Dear Colleagues,

We have prepared a report in response to the DOE's recent call for public comments on "*A Critical Review of Impacts of Greenhouse Gas Emissions on the U.S. Climate.*"

Our goal is to provide additional scientific context—offered as further reading—on the intricate relationships among tipping points, bifurcation points, and multistability. These topics are of particular interest because our 2023 study is directly cited in the DOE report's section on tipping points. We believe the clarifications and supplemental material will help ensure accuracy and deepen the scientific understanding presented, while also highlighting the complexity of the climate system and the breadth of mathematical tools available for its analysis.

A draft version, to be finalized next week, is available here:

Shen, B.-W. and R. Pielke Sr., 2025: *Tipping, Bifurcation, and Multistability: Insights from the Generalized Lorenz Model.*

<http://doi.org/10.13140/RG.2.2.30493.01763>

Your comments are greatly appreciated. Have a wonderful weekend!

Best,

-Bowen

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Bo-Wen Shen, Ph.D.

Department of Mathematics and Statistics

San Diego State University

Web: <https://bwshen.sdsu.edu>

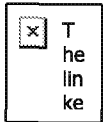
Tropical-storms mailing list

Tropical-storms@lists.tstorms.org

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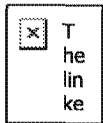


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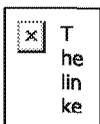
Bo-Wen Shen, Ph.D.
Department of Mathematics and Statistics
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Web: <https://bwshen.sdsu.edu>

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Judith Curry, President
CFAN - Climate Forecast Applications
Network
Reno, NV USA
curry.judith@cfanclimate.com |
+1.404.803.2012
<http://www.cfanclimate.net>

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Judith Curry, President
CFAN - Climate Forecast Applications
Network
Reno, NV USA
curry.judith@cfanclimate.com |
+1.404.803.2012
<http://www.cfanclimate.net>

11.2.3 Evidence for low SCC

Chapter 2 reviewed evidence on climate change and greening, and Chapter 9 looked at climate change and U.S. agriculture. Evidence shows that CO₂ fertilization has a stronger beneficial effect on agriculture than was known at the time that IAMs like DICE and FUND were parameterized. Haverd *et al.* (2020) found the observed CO₂ fertilization rate has been almost double what had been predicted by crop models. Dayaratna *et al.* (2023) used the updated empirical ECS distribution estimate of Lewis (2022), which assimilates modern instrumental and paleoclimatic temperature records, and allowed for a 30 percent gain in the CO₂ fertilization benefit in the FUND model, and found that, even at a low discount rate of 2.5 percent, the median SCC as of 2050 is only \$18.67, with a 24 percent probability of the true value being negative. At a five percent discount rate the median SCC value is negative until the mid-2040s and at 2050 was only \$0.37 with a 49 percent probability of being negative. Thus, under reasonable assumptions a mainstream IAM using updated scientific inputs yields evidence consistent with the SCC not being significantly greater than zero.

It should also be noted that the SCC is focused on the social costs of CO₂ emissions from fossil fuel use. It is not intended to measure the private marginal benefits to consumers and society from the availability of fossil fuels. Public willingness to pay for fuels of all types indicates the value to society of reliable, abundant fossil energy. Tol (2017) estimates that the private benefit of carbon is large relative to the social cost. This can be illustrated by noting that the price of a gallon of gas indicates the marginal value to the consumer of the fuel. Suppose we assume a relatively high Social Cost of Carbon of, say, \$75 per tonne. Deflated by a MCPF⁴ value of 1.5 that would result in a carbon tax of \$50 per tonne, which equates to about 44 cents per gallon of gas (Lavelle, 2019). A pre-tax price of \$3.00 per gallon would imply the marginal social benefit of the fuel is nearly seven times the marginal social cost.

11.2.4 Tipping points

SCC calculations typically consider gradual impacts of a warming climate, such as slowly melting glaciers and increasing average temperature. A driver of potentially high values of the Social Cost of Carbon (SCC) is the introduction into models of discrete catastrophic outcomes associated with abrupt changes (Dietz *et al.*, 2021). They are often referred to as “tipping points.”

The term “tipping point” mingles two different physical concepts that pose different research challenges. Many physical systems are inherently stable unless acted upon with sufficient external energy. For example, an ice cap might remain intact over a wide range of temperatures but once the temperature crosses the 0°C threshold it melts. Such discontinuities are ubiquitous in nature and require an external force. Whether the force needs to be large relative to the size of the system depends on the underlying stability of the system.

A different type of tipping point is called a *bifurcation* and arises from the study of the internal dynamics of nonlinear systems (Crawford, 1991). Many systems have been observed to have more than one equilibrium point and can move between them with minimal or no external influence. For example, a weather system might have two different equilibrium states: one calm and one with a tornado. A transition from one to another can happen either with no external force or with a minuscule change, such as a flap of the proverbial butterfly’s wings (Shen *et al.* 2014). The term “tipping point” is sometimes used to mean a bifurcation of this type and implies instability inherent in the system itself, which is not necessarily dependent on outside forces. It depends, instead, on parameters of the system taking values that support the emergence of bifurcations (Crawford, 1991).

The two different concepts imply different research questions. Regarding the first we are interested in whether components of the climate system are susceptible to abrupt discontinuities in response to

⁴ Marginal Cost of Public Funds: the optimal carbon tax rate is the SCC divided by the MCPF (Sandmo 1977).

sufficiently large anthropogenic forcing. Regarding the second we are interested in whether the Earth's climate system has inherent bifurcations that imply the possibility of abrupt transitions with or without external forcing.

Models have been developed that imply the second type is a possibility. Kypke *et al.* (2020) presented a simple climate model in which the GHG concentration is one of the parameters that controls the emergence of bifurcations of the Arctic climate to one with both cold and warm equilibria. If sufficiently high GHG forcing combined with a sufficiently high rate of ocean heat transport are imposed a bifurcation becomes possible. More generally GCMs have been observed to contain bifurcations and multiple equilibria (Brunetti and Ragon, 2023).

The possible existence of bifurcations in the Earth's climate system implies abrupt transitions are possible, not just in response to large forcing but also to small perturbations. This places tipping points into the category of low-likelihood and potentially catastrophic events, such as large meteor strikes. A key question to ask is whether those kinds of tipping points can be predicted. Current research has not resolved that question (Dakos *et al.*, 2024) and indeed might not be able to since one implication of the “butterfly effect” is the existence of predictability boundaries of nonlinear systems (Palmer *et al.*, 2014). It is therefore not obvious how to incorporate such possibilities into SCC calculations. Small variations in assumptions will lead to arbitrarily large variations in the resulting SCC with no grounds for choosing among them. If such tipping points are possible the most appropriate stance for economic policy is to maximize resilience to any form of external catastrophe since it is unlikely we could predict it or prevent it from happening.

AR6 (WGI, Chapter 1) focuses mainly on the first type of tipping point, namely an abrupt change in response to external forcing. This is also the meaning associated with popular usage of the “tipping point” concept in discussions of climate change (see <https://report-2023.global-tipping-points.org/what-is-a-tipping-point/> for example). As summarized by AR6, there is evidence of abrupt change in the paleoclimate record, and some of these events have been interpreted as tipping points. Some projections with Earth System Models for example have produced tipping points such as Amazon forest dieback in response to specified values of CO₂ concentration or temperature increases.

The alarm surrounding climate tipping points is reflected by *The Global Tipping Points Report* that was launched at COP28 on 6 December 2023 (Lenton *et al.*, 2023). It identifies more than 25 parts of the climate system said to constitute tipping points. What gets classified as a climate “tipping point” is a moving target. The most common examples in the literature and assessment reports include: Greenland ice sheet disintegration, West Antarctic ice sheet disintegration, summertime disappearance of Arctic sea ice, Amazon rainforest dieback, coral reef dieoff, thawing of permafrost and methane hydrates, Atlantic Meridional Overturning Circulation collapse, boreal forest shift, West African monsoon shift, and Indian Monsoon shift.

All such tipping points require a certain amount of system instability to exhibit an abrupt transition in response to warming. For this reason, there seems to be very little to discriminate between a tipping event and natural climate variability. Natural climate variability has in the past produced shifts in the West African and Indian monsoons, Amazon forest and coral reef dieback, and disintegration of parts of the Greenland and West Antarctic ice sheets. These impacts can reverse on the decadal and century timescales associated with natural climate variability and ecosystem responses.

Some abrupt changes are potentially more consequential, including collapse of the West Antarctic ice sheet and collapse of the Atlantic Meridional Overturning Circulation (AMOC). AR6 WG1 Summary for Policy Makers states:

The Atlantic Meridional Overturning Circulation is very likely to weaken over the 21st century for all emission scenarios. While there is *high confidence* in the 21st century decline, there is only *low*

confidence in the magnitude of the trend. There is *medium confidence* that there will not be an abrupt collapse before 2100. (C.3.4)

There is limited evidence for low-likelihood, high-impact outcomes (resulting from ice-sheet instability processes characterized by deep uncertainty and in some cases involving tipping points) that would strongly increase ice loss from the Antarctic Ice Sheet for centuries under high GHG emissions scenarios (e.g., SSP5-8.5). (B.5.2)

For SCC calculations, the research question implied by this type of tipping point is whether such events have been observed in the past in climate conditions similar to what we currently experience or will in the near future. AR6 finds little evidence for impending collapse of the Atlantic Meridional Overturning Circulation or the West Antarctic ice sheet. It finds there is no tipping point associated with Arctic Sea ice (AR6 Technical Summary p. 76)

Dietz *et al.* incorporated several potential tipping points (abrupt changes) into an SCC model and found they added about 25 percent to the estimate, mainly associated with thawing permafrost and release of methane hydrates. However, the IPCC considers this scenario *very unlikely* (AR6 Technical Summary p. 107).

In summary, there might be unknown bifurcation tipping points that are associated with natural climate processes, but this possibility does not translate into specific guidance on the SCC. There are potential abrupt change points in the climate system in response to warming, although the IPCC assigns low probabilities to most, including the largest ones. When these have been considered, the result is only a modest increase in the SCC value in the 21st century.

11.2.5 Are there alternatives?

It is increasingly being argued that the SCC is too variable to be useful for policymakers. Cambridge Econometrics (Thoung, 2017) stated it's "time to kill it" due to uncertainties. The UK and EU no longer use SCC for policy appraisal, opting for "target-consistent" carbon pricing (UK Department for Energy Security and Net Zero 2022, Dunne 2017). However, the uncertainty of SCC estimates doesn't mean that other regulatory instruments are inherently better or more efficient. Many emissions regulations (such as electric vehicle mandates, renewable energy mandates, energy efficiency regulations and bans on certain types of home appliances) cost far more per tonne of abatement than any mainstream SCC estimate, which is sufficient to establish that they fail a cost-benefit test.

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From: Judith Curry <curry.judith@gmail.com>
Sent: Saturday, August 16, 2025 11:38 AM
To: Ross McKittrick
Subject: Re: [Tropical-storms] DOE Report – Discussions on Tipping Points, Bifurcation, and Multistability

ok, they said they are submitting a specific comment to the DOE portal

On Sat, Aug 16, 2025 at 8:35 AM Ross McKittrick <ross.mckittrick@gmail.com> wrote:
The paper just describes a lot of intro level concepts but other than correcting the date on a citation doesn't mention cwg25 and doesn't challenge anything therein.

On Sat, Aug 16, 2025 at 7:58 AM Ross McKittrick <ross.mckittrick@gmail.com> wrote:
Thanks I'll look.

On Sat, Aug 16, 2025 at 6:12 AM Judith Curry <curry.judith@gmail.com> wrote:
Hi Ross, this may be of relevance to ch 11

----- Forwarded message -----

From: Stoffelen, Ad (KNMI) via Tropical-storms <tropical-storms@lists.tstorms.org>
Date: Sat, Aug 16, 2025 at 3:56 AM
Subject: Re: [Tropical-storms] DOE Report – Discussions on Tipping Points, Bifurcation, and Multistability
To: Bo-Wen Shen <bwshen762@gmail.com>, Tropical-Storms <Tropical-storms@lists.tstorms.org>

Dear Bowen,

It is much appreciated that you try and provide further scientific understanding of the consequences of Greenhouse gas emissions. On the other hand, we already have ten thousands of peer-reviewed manuscripts providing understanding and through peer-reviewing by experts a sound view of these consequences. How would this single simplified study help the scientists that were involved in the DOE report?

No doubt your manuscript can be a pearl among all the others, while I believe it is not scientific understanding that is the prime topic of the DOE report.

Cheers,

Ad

From: Tropical-storms <tropical-storms-bounces@lists.tstorms.org> **On Behalf Of** Bo-Wen Shen via Tropical-storms
Sent: zaterdag 16 augustus 2025 00:21
To: Tropical-Storms <Tropical-storms@lists.tstorms.org>
Subject: [Tropical-storms] DOE Report – Discussions on Tipping Points, Bifurcation, and Multistability

Dear Colleagues,

We have prepared a report in response to the DOE's recent call for public comments on "*A Critical Review of Impacts of Greenhouse Gas Emissions on the U.S. Climate.*"

Our goal is to provide additional scientific context—offered as further reading—on the intricate relationships among tipping points, bifurcation points, and multistability. These topics are of particular interest because our 2023 study is directly cited in the DOE report's section on tipping points. We believe the clarifications and supplemental material will help ensure accuracy and deepen the scientific understanding presented, while also highlighting the complexity of the climate system and the breadth of mathematical tools available for its analysis.

A draft version, to be finalized next week, is available here:

Shen, B.-W. and R. Pielke Sr., 2025: *Tipping, Bifurcation, and Multistability: Insights from the Generalized Lorenz Model.*

<http://doi.org/10.13140/RG.2.2.30493.01763>

Your comments are greatly appreciated. Have a wonderful weekend!

Best,

-Bowen

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Bo-Wen Shen, Ph.D.

Department of Mathematics and Statistics

San Diego State University

Web: <https://bwshen.sdsu.edu>

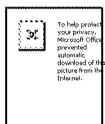
Tropical-storms mailing list

Tropical-storms@lists.tstorms.org

<http://tstorms.org/>

Please do not forward this message to non-qualified individuals.

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Judith Curry, President

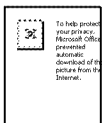
CFAN - Climate Forecast Applications Network

Reno, NV USA

curry.judith@cfanclimate.com | +1.404.803.2012

<http://www.cfanclimate.net>

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Judith Curry, President

CFAN - Climate Forecast Applications Network

Reno, NV USA

curry.judith@cfanclimate.com | +1.404.803.2012

<http://www.cfanclimate.net>

From: Judith Curry [curry.judith@gmail.com]
Sent: 8/13/2025 10:15:18 PM
To: Steve Koonin [steven.koonin@gmail.com]; Roy Spencer [roywspencer@hotmail.com]; John Christy [climateman60@gmail.com]; Ross McKittrick [ross.mckittrick@gmail.com]
Subject: climate sensitivity
Attachments: Palmer News and Views Williams et al.pdf

Here is a summary of the climate sensitivity discussion with Tim Palmer, see also attached article. Thoughts on whether to include this (i'm inclined not to).

On 13 Aug 2025, at 20:38, Judith Curry <curry.judith@gmail.com> wrote:

Here is why i don't buy this argument. Processes that are important on climate scales are of different importance from the weather scale. For example, the fix to the UKMO model related to dual moment microphysics definitely improves the fidelity of the precip process, and maybe even cloud fraction/distribution (as far as i understand, this is the main contributor to very high ECS value). If the models have too much aerosol (i.e. inadequate aerosol sink), no one cares on the weather time scale but it is huge issue on the climate time scale. Also, changes to simulated convection (combined with oversimplified moist thermodynamics, which is the case with all climate models last time i checked with CMIP5) can cause unrealistic accumulated water vapor in upper troposphere; no one cares on weather time scale, but a huge issue on climate time scales.

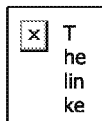
Of course, there are plenty of processes that are important on the climate timescale that are not important on the weather timescale. That's not my point.

My point is that If you have two possible representation of a process that contributes to climate sensitivity and one produces significantly better weather forecasts and the other significantly worse, better to go with the one that produces good weather forecasts.

I am certainly not saying that this is proof that climate sensitivity is unquestionably high. Of course not. It's just a "line of evidence" to offset others used e.g. by Nic.

Tim

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Judith Curry, President
CFAN - Climate Forecast Applications Network
Reno, NV USA
curry.judith@cfanclimate.com | +1.404.803.2012
<http://www.cfanclimate.net>

News & views

Climate change

Short-term tests for long-term estimates

Tim Palmer

Six-hour weather forecasts have been used to validate estimates of climate change hundreds of years from now. Such tests have great potential – but only if our weather-forecasting and climate-prediction systems are unified.

How sensitive is climate to atmospheric carbon dioxide levels? For a doubling of CO₂ concentration from pre-industrial levels, some models predict an alarming long-term warming of more than 5 °C. But are these estimates believable? Writing in the *Journal of Advances in Modeling Earth Systems*, Williams *et al.*¹ have tested some of the revisions that have been made to one such model by assessing its accuracy for very short-term weather forecasts. The results are not reassuring – they support the estimates.

There is little doubt, at least among those who understand the science, that climate change is one of the greatest challenges facing humans in the coming decades. However, the extent to which unchecked climate change would prove catastrophic rests on processes that are poorly understood. Perhaps the most important of these concern the way in which Earth's hydrological cycle – which includes the evaporation, condensation and movement of water – will react to our warming planet.

One of the key problems is how clouds adjust to warming². If low-level cloud cover increases, and high-level clouds decrease, then clouds will offset the warming effect of increased atmospheric CO₂ concentrations and thereby act as a negative feedback, or damper, on climate change, buying us some breathing space. By contrast, if there is positive cloud feedback – that is, if low-level clouds decrease with warming and high-level clouds increase – then, short of rapid and complete cessation of fossil-fuel use, we might be heading for disaster.

So what have clouds been doing as global warming has slowly taken hold? Trends in global cloud cover can be estimated only from space-based observations (Fig. 1). However, cloud data sets derived from multiple satellites

over several decades suffer from spurious artefacts related to changes in satellite orbit, instrument calibration and other factors³. These artefacts are particularly large when estimating globally averaged cloud cover, currently preventing any reliable estimation of trends in one direction or the other.

In lieu of observational evidence, we must turn to computational models of the climate system. But there is a problem. Clouds are on too small a scale to be represented using the laws of physics in current climate models. Instead, they are represented by relatively

crude, computationally cheap bulk formulae known as parameterizations. These do encode some basic ideas of cloud physics – clouds' dependence on the ambient temperature, humidity and vertical air velocity, for example – but they are far from being *ab initio* estimates. Hence, the role of clouds in climate change is crucial but uncertain⁴.

The cloud-feedback problem has been brought sharply into focus in recent months as results have been emerging from the dozens of climate-change models in an ensemble called the Coupled Model Intercomparison Project (CMIP6; see go.nature.com/3garyzc). Projections of future climate from this global effort have fed into the Sixth Assessment Report of the Intergovernmental Panel on Climate Change (IPCC), due next year.

Some of the latest-generation models in CMIP6 now indicate climate sensitivities exceeding 5 °C (refs 5–7). Here, climate sensitivity refers to the global warming after climate has equilibrated to a doubling of CO₂ concentration relative to pre-industrial levels, an equilibrium that might take a few hundred years to establish⁸. These sensitivity values are outside the range of those produced by the CMIP5 ensemble, which fed into the previous IPCC Assessment Report⁹ in 2013. They seem to have arisen largely because of revisions to how cloud microphysics is represented,

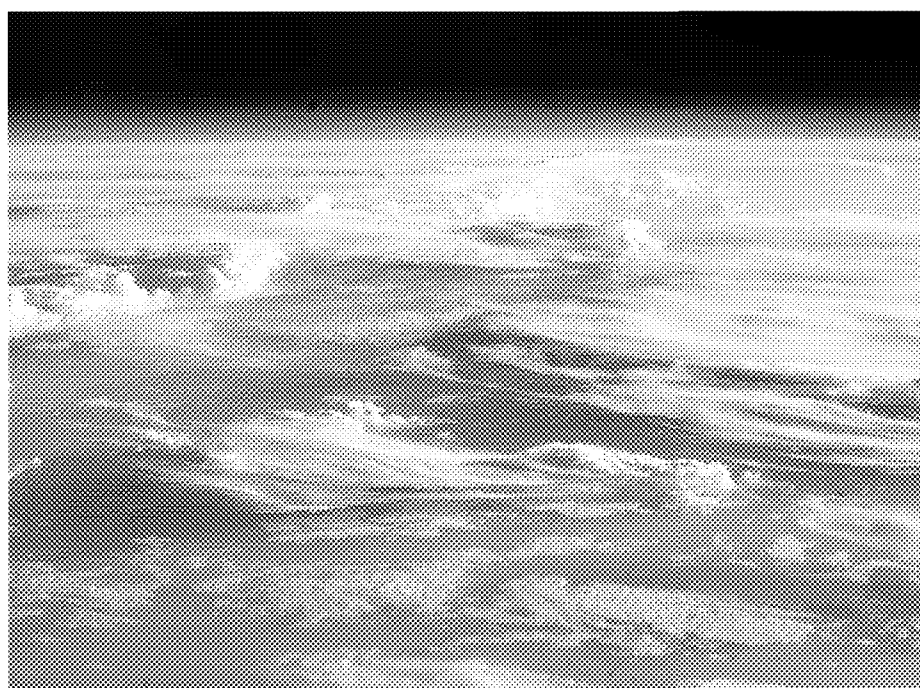


Figure 1 | Cloudy skies, viewed from space. How clouds will adjust to a warming climate is difficult to predict, but Williams *et al.*¹ have used short-term weather forecasts to assess whether recent revisions to long-term climate models are getting us nearer to the truth.

particularly in the parameterization of supercooled liquid water. Cloud microphysics describes the properties (such as size and relative concentration) of water and ice droplets in a cloud. On such tiny matters might our future rest.

And so, the question is this. Are we to believe these new estimates of climate sensitivity, or will they end up being reverted to earlier CMIP5 values as the models go through a further round of revisions?

Some years ago, the meteorologist Mark Rodwell and I proposed⁹ a method for assessing predictions of climate sensitivity – one based on very short-range (6-hour) weather forecasts. We were motivated by startling results suggesting that warming could be as much as 11 °C for a doubling of CO₂ levels¹⁰. These high estimates arose in climate models in which a particular cloud-system parameter, known as convective entrainment, was set to unusually small values that could not readily be ruled out by studying the accuracy of the models' climate simulations. By showing that errors of 6-hour weather forecasts were made substantially worse using a model with these reduced values of convective entrainment, we were able to cast doubt on the credibility of these exceptionally large estimates of climate sensitivity.

We found that if we ran a state-of-the-art numerical weather-prediction system with a low convective entrainment parameter, it produced much less accurate 6-hour forecasts than when the forecast model had more-typical values plugged in. To everyone's relief, this suggested that the low values of the parameter used in the climate models were unrealistic, and thus we could discount the alarming 11 °C sensitivity estimates.

Williams *et al.* have now subjected the CMIP6 Met Office climate model to the same 6-hour

weather-forecast test. The authors chose to test this model because it was one of those that produced a relatively large climate sensitivity of about 5.5 °C. The model has a revised scheme for cloud microphysics as mentioned above, in which there are more supercooled water droplets and fewer ice droplets.

The authors found that the 6-hour-forecast errors were smaller for the revised model than for a version of the model without the cloud-microphysics revisions. Hence, instead of being able to discount estimates of high sensitivity, as Rodwell and I had done, their result provides some of the best current evidence that climate sensitivity could indeed be 5 °C or greater.

In short, these results, published in a specialist journal, and probably read by few climate policymakers, carry a far-reaching message: we cannot afford to be complacent. It seems that cloud adjustment to climate change is not going to give us breathing space. Instead, we need to redouble our efforts to cut emissions.

There is a serious caveat to the general application of this technique. The test makes sense only if the model used to do the short-term forecast is the same as the one used to do the climate projection. The Met Office weather and climate models are reasonably similar (their model is often called the 'Unified Model'), but weather models do not generally correspond well with climate models.

On top of this, an accurate 6-hour weather forecast is possible only if one can come up with accurate initial conditions for the model from observations, a process known as data assimilation. This is a complex and computationally demanding optimization problem¹¹, and most climate institutes do not have such data-assimilation capability. Moreover, accurate data assimilation requires the spatial and time resolution of climate models

to be increased to be comparable with those used for state-of-the-art weather forecasting. Conversely, the parameterizations in weather-forecast models must be as complex and comprehensive as the ones in corresponding climate models; few weather-forecast centres have the resources for this.

Thus, to reduce uncertainty in estimates of the crucial cloud feedbacks, climate institutes and weather-forecast centres should work together to ensure that their model systems are as seamless^{12,13} as possible. I contend that weather and climate modelling must be rationalized worldwide, and that human and computational resources should be pooled to produce high-resolution, unified weather–climate models^{14,15}.

Tim Palmer is in the Department of Physics, University of Oxford, Oxford OX1 3PU, UK.
e-mail: tim.palmer@physics.ox.ac.uk

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From: Roy Spencer <roywspencer@hotmail.com>
Sent: Tuesday, August 5, 2025 10:28 AM
To: Judith Curry
Cc: Steven Koonin; climateman60@gmail.com; ross.mckitrick@gmail.com
Subject: Re: what has happened to Jennifer Marohasy?

I've studied this some (and oceanography was part of my first degree from U. MI).

1. The lower half of the oceans averages very cold, everywhere, around 3 - 4 deg. C.
2. They have towed sensor arrays across the mid-ocean ridges, even over the "smokers", and can't find any localized warming... the heat sources are simply too weak and get rapidly mixed away.
3. The Argo floats show virtually no warming at 1,500 to 2,000 m depth, consistent with warming from above very slowly mixing downward.

4) Even if current estimates of geothermal heat flux were to suddenly increase by 10x, we wouldn't be able to measure any change in the deep oceans for decades.

-Roy

From: Judith Curry <curry.judith@gmail.com>
Sent: Tuesday, August 5, 2025 8:05 AM
To: Roy Spencer <roywspencer@hotmail.com>
Cc: Steven Koonin <steven.koonin@gmail.com>; climateman60@gmail.com <climateman60@gmail.com>; ross.mckitrick@gmail.com <ross.mckitrick@gmail.com>
Subject: Re: what has happened to Jennifer Marohasy?

well I take the warming from the ocean pretty seriously. It is a factor, and we don't know enough about it.

On Tue, Aug 5, 2025 at 12:59 AM Roy Spencer <roywspencer@hotmail.com> wrote:
Jennifer has been blogging some crazy stuff lately... warming is due to volcanoes in the deep ocean; increasing CO2 is due to outgassing from the oceans. So has Joe Bastardi.

Here's her new Facebook post chiding us and the DOE report for being part of the current global warming "conspiracy" "paradigm":

"President Trump's Energy Department published a report last Tuesday essentially endorsing the human-caused global warming carbon-conspiracy. It is not being reported as such, it is being reported as authored by climate deniers who are attacking climate science, but that is just gaslighting or is it an inability to understand the limitations of the current paradigm.

The new DOE report entitled 'A Critical Review of Impacts of Greenhouse Gas Emissions on the US Climate', authored by John Christy, Judith Curry, Steven Koonin, Ross McKittrick and Roy Spencer, has surprised me both in its scope and the extent to which it is essentially an endorsement of the current failed paradigm that climate change is human-caused.

The Christy et al. report operates within the existing paradigm, critiquing IPCC excesses but not proposing any new hypotheses or methods. Worse, while claiming to be data driven and critical of general circulation modelling, it continually reverts to output from general circulation models to draw conclusions, ignoring studies that provide experimental measurements including for calculating Equilibrium Climate Sensitivity (ECS).

The day after the report was released in the USA with fake accusations that this was an attack on mainstream climate science, here in Australia a new parliamentary committee was established ostensibly to prevent disinformation on climate science. I've been told that my blog, my Substack, and this Facebook page are part of an aggressive and coordinated disinformation campaign on climate change. In fact, I have been proposing an alternative paradigm, that undermines the foundation of human-caused climate change theory. For over two decades, I have been simply attempting a more evidence-based approach to climate science specifically through the application of artificial neural networks for rainfall forecasting. I have continually emphasised that the reasonable test of the value of any scientific theory is its utility, not the number of institutions that subscribe to it. More information and links to the various reports and disinformation committees across at my Substack. "

From: Steven Koonin <steven.koonin@gmail.com>

Sent: Monday, August 4, 2025 4:30 PM

To: roywspencer@hotmail.com <roywspencer@hotmail.com>; climateman60@gmail.com <climateman60@gmail.com>; ross.mckittrick@gmail.com <ross.mckittrick@gmail.com>; 'Judith Curry' <curry.judith@gmail.com>

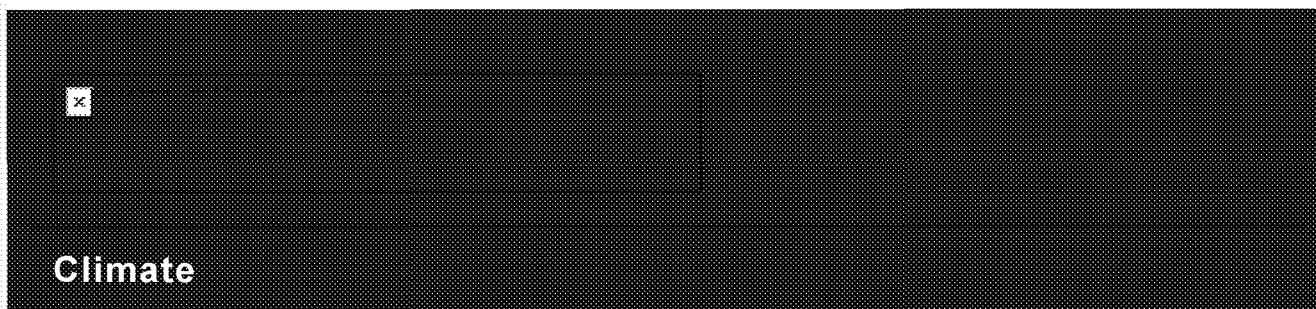
Subject: NASEM on climate assessments

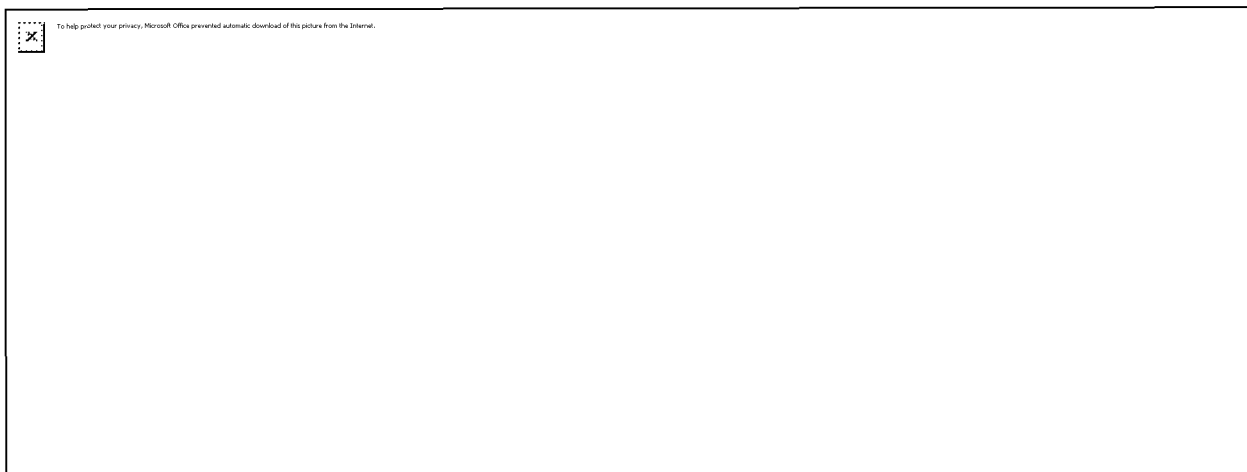
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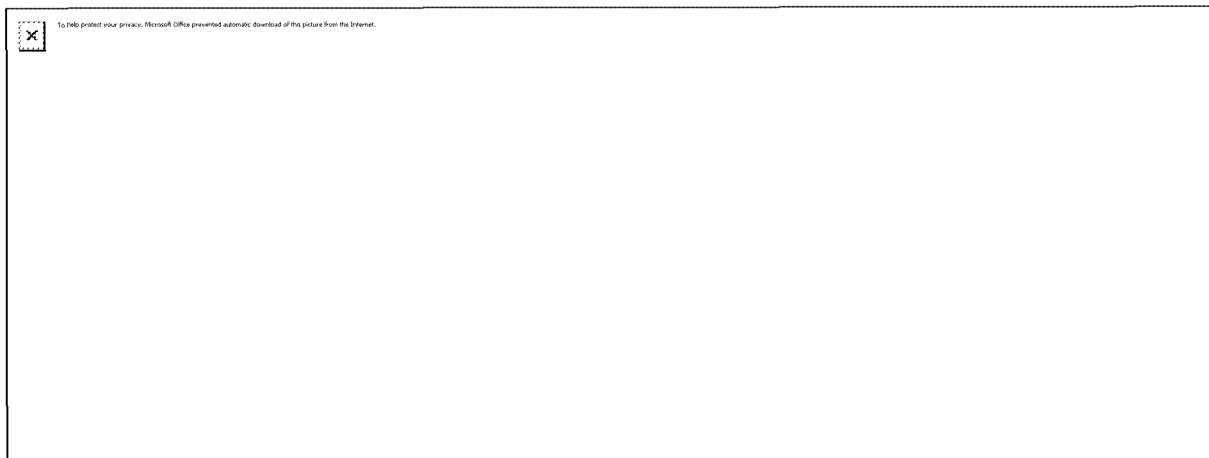




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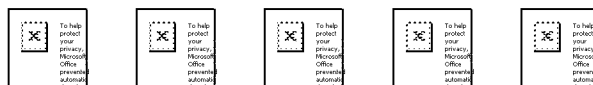
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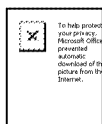


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<http://www.cfanclimate.net>

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Sent: 8/5/2025 2:04:41 PM
To: Judith Curry [curry.judith@gmail.com]
CC: Roy Spencer [roywspencer@hotmail.com]; Steven Koonin [steven.koonin@gmail.com]; ross.mckitrick@gmail.com
Subject: Re: what has happened to Jennifer Marohasy?

Huh? I guess Ross had to be replaced by a US guy

<https://myemail.constantcontact.com/New-Report-Rocks-the-Climate-Industrial-Complex-to-its-Core.html?soid=1101509381788&aid=Xi4V4Xk5HQ8>

Last week, the Department of Energy issued a report that is a must-read for all receiving this newsletter:

A Critical Review of Impacts of Greenhouse Gas Emissions on the U.S. Climate.

The report fully aligns with the CO2 Coalition's many analyses and was written by five respected scientists: John Christy, David Legates, Steve Koonin, Judith Curry and CO2 Coalition member Roy Spencer.

John C.

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+1.404.803.2012
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Cc: Steven Koonin; climateman60@gmail.com; ross.mckitrick@gmail.com
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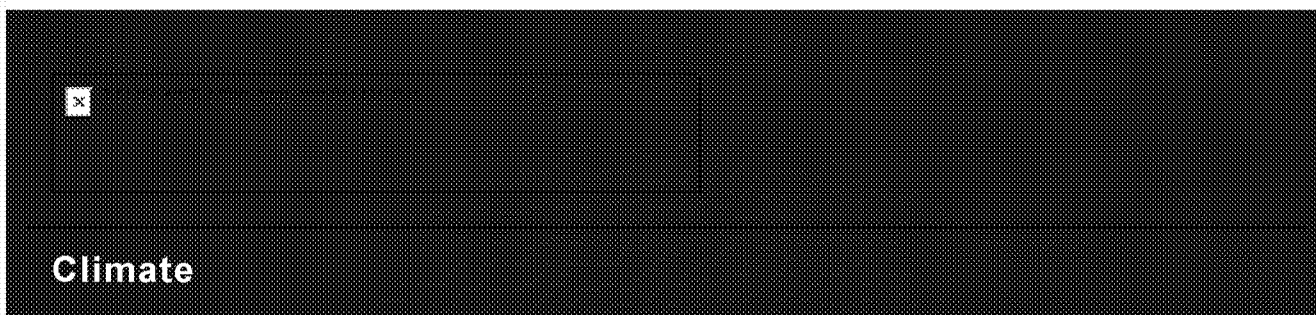
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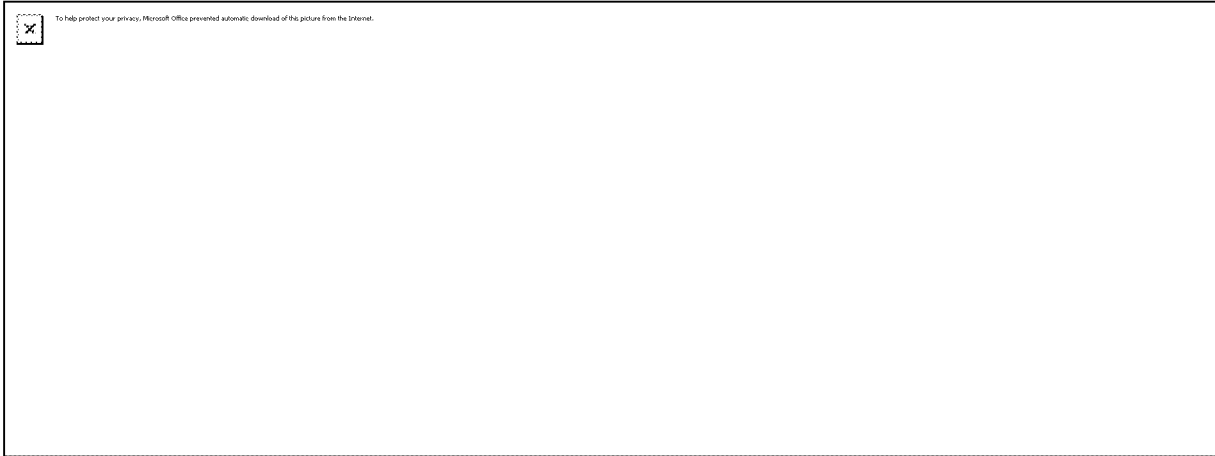


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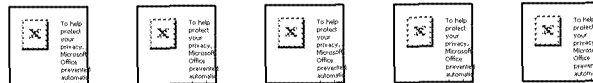
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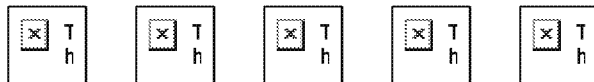
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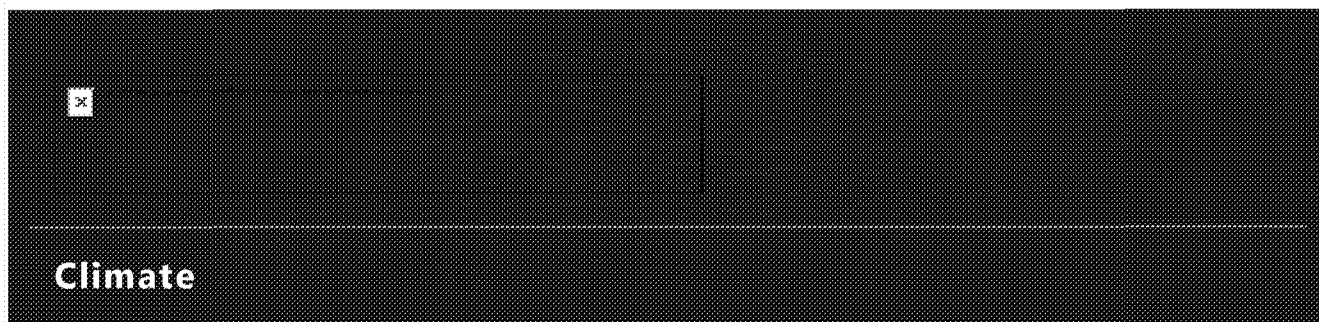
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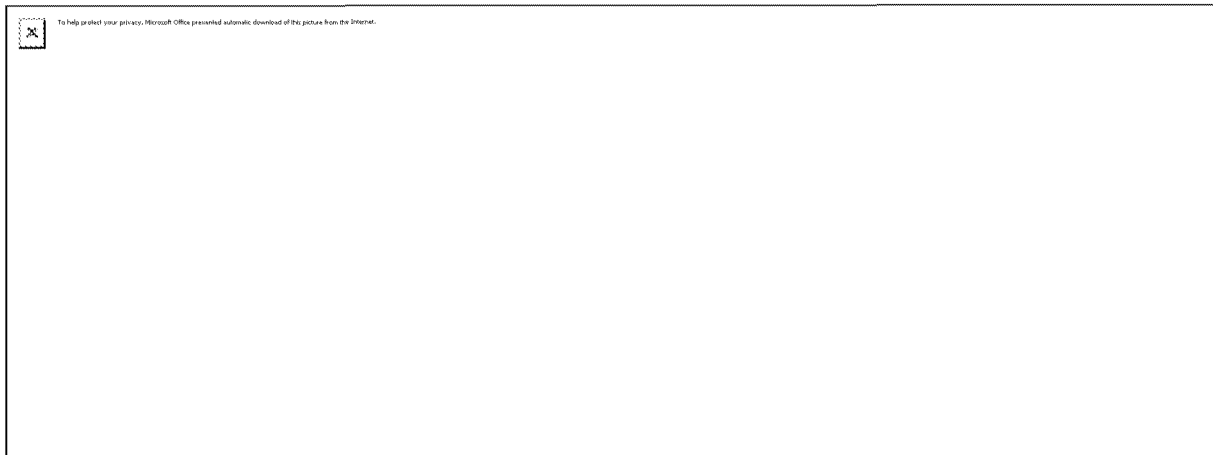
Week of August 4, 2025



Climate assessments are critical processes that communicate scientific findings to inform decision-making. In the second of a series of climate science retrospectives, [discover the pivotal role](#) the National Academies has played in advancing national climate assessments. Explore more National Academies [climate assessment resources and publications](#) below.

Keep reading to learn about our [relevant resources](#), [upcoming events](#), and [the latest](#) from the Academies.

Resources & Publications



Program News

Advancing Climate Assessments: 50 Years of Progress and Impact

Released July 2025

As our understanding of climate change grew over the last 50 years, efforts to communicate what is known and the implications for decision making have emerged and evolved hand-in-hand. Climate assessments—conducted by governments, international bodies, and non-governmental organizations—are critical processes for

bringing the scientific community together with those who use their findings to produce trusted decision-relevant information.

The National Academies have played pivotal roles in advancing climate assessments. Many of the reports featured in our [first retrospective](#) dating back to the 1970s and 1980s could be considered assessments, in that they brought together leading experts to review the literature and summarize what was known in a manner that could inform policy and other decisions. These early reports helped demonstrate the value of regular updates on the state of the science, especially for a rapidly-evolving field that integrates many kinds of knowledge.

[READ THE FULL ARTICLE](#)

More resources and publications

Consensus Report: [Developing a Strategy to Evaluate the National Climate Assessment](#)

Released September 2024

Consensus Report: [Review of the Draft Fifth National Climate Assessment](#)

Released August 2023

Workshop Proceedings: [Making Climate Assessments Work: Learning from California and Other Subnational Climate Assessments](#)

Released January 2019

Consensus Report: [Review of the Draft Interagency Report on the Impacts of Climate Change on Human Health in the United States](#)

Released August 2015

Consensus Report: Informing Decisions in a Changing Climate

Released August 2009

Upcoming Events

Webinar: AI, Energy Demand, and the Environment: Perspectives on Information Sharing

August 15, 2025 | Virtual

Webinar: Strategies for Integrating AI into State and Local Government Decision Making Webinar

August 21, 2025 | Virtual

Climate Conversations: Educational Pathways for a Climate-Ready Workforce

August 22, 2025 | Virtual

Webinar: Value-Added Products and Co-Benefits from GHG Removals

August 28, 2025 | Virtual

Climate Conversations: Air Pollution in India

September 29, 2025 | San Diego, CA & Virtual

Just In!

Announcements:

- ? Plugging Americas Forgotten Wells - National Academies Study Addresses Decades Long Problem: *Program News*

New Releases

- ? Higher Education's Path to Sustainability and Resilience: *Workshop Proceedings*

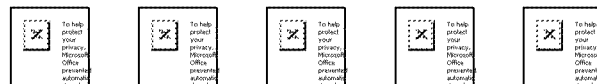
- ? [Strategies for Integrating AI into State and Local Government Decision Making: Rapid Expert Consultation](#)

Opportunities

- ? [New Voices in Sciences, Engineering and Medicine: Call for Applications](#)
- ? [Electricity Forum: Call for Abstracts](#)
- ? [The Christine Mirzayan Science & Technology Policy Graduate Fellowship Program: Call for Applications](#)

See [Climate at the National Academies](#) for more about our upcoming events, current work, and existing resources.

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From: John Christy <climateman60@gmail.com>
Sent: Thursday, August 7, 2025 8:16 PM
To: Roy Spencer
Cc: Steve Koonin; Ross McKittrick; Judith Curry; Josh Loucks; Woods, Andrea; Ison, Jeremy T.; Travis Fisher
Subject: Re: and now NASEM gets into the act

Steve

I don't know how the NASEM works these days. Will they have a broad view of which authors will be chosen, to include people who have experience with data and large non-linear dynamical systems (like physicists)? Or will the authors be chosen by the Board of Atmospheric Sciences and Climate?

It seems to me the announcement was directed toward the EF (i.e. "reasonably anticipated to endanger public health and welfare in the US") which can be a very squishy mandate (what do "reasonably" and "endanger" mean?). This doesn't seem to be a basic, dispassionate analysis of the science itself.

John C.

John Christy
Director, Earth System Science Center
Professor, Atmos and Earth Science
Alabama State Climatologist
The University of Alabama in Huntsville
climateman60@gmail.com

On Aug 7, 2025, at 4:20 PM, Roy Spencer <roywspencer@hotmail.com> wrote:

We already know what their report will say. And it will receive glowing reviews. But I'll bet I can guess why they are suddenly doing this. :-) -Roy

Sent from my Verizon, Samsung Galaxy smartphone

----- Original message -----

From: Steven Koonin <steven.koonin@gmail.com>
Date: 8/7/25 3:43 PM (GMT-06:00)
To: 'John Christy' <climateman60@gmail.com>, 'Ross McKittrick' <ross.mckittrick@gmail.com>, 'Judith Curry' <curry.judith@gmail.com>, "'Roy W. Spencer'" <roywspencer@hotmail.com>
Cc: 'Josh Loucks' <loucksj14@gmail.com>, "'Woods, Andrea'" <andrea.woods@hq.doe.gov>, "'Ison, Jeremy T.'" <jeremy.ison@hq.doe.gov>, 'Travis Fisher' <travis.scott.fisher@gmail.com>
Subject: and now NASEM gets into the act

<https://www.nationalacademies.org/our-work/anthropogenic-greenhouse-gases-and-us-climate-evidence-and-impacts>

I think this is a good thing – it will open the dialog.

Question is whether the resultant report will receive the same scrutiny our report has gotten.

Steve Koonin

From: Roy Spencer <roywspencer@hotmail.com>
Sent: Thursday, August 7, 2025 5:21 PM
To: Steven Koonin; 'John Christy'; 'Ross McKittrick'; 'Judith Curry'
Cc: 'Josh Loucks'; 'Woods, Andrea'; 'Ison, Jeremy T.'; 'Travis Fisher'
Subject: RE: and now NASEM gets into the act

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Cc: 'Josh Loucks' <loucksj14@gmail.com>, "'Woods, Andrea'" <andrea.woods@hq.doe.gov>, "'Ison, Jeremy T.'" <jeremy.ison@hq.doe.gov>, 'Travis Fisher' <travis.scott.fisher@gmail.com>
Subject: and now NASEM gets into the act

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Steve Koonin

From: Ross McKittrick <ross.mckitrick@gmail.com>
Sent: Thursday, August 7, 2025 5:10 PM
To: Steven Koonin
Cc: John Christy; Judith Curry; Roy W. Spencer; Josh Loucks; Woods, Andrea; Ison, Jeremy T.; Travis Fisher
Subject: Re: and now NASEM gets into the act

Deadline for submissions is Aug 27.

According to the press release (<https://x.com/JunkScience/status/1953549045248491887>) it "will be completed and publicly released in September".

So no, it will not receive anything like the same level of scrutiny as our report.

This looks to me like they're injecting themselves into a political battle. "Endangerment" is not a scientific question, it's a legal/value judgment, and no one asked Marcia McNutt to weigh in. I think there's 0% chance they will come back with any other answer than that GHG emissions endanger the public.

On Thu, Aug 7, 2025 at 4:43 PM Steven Koonin <steven.koonin@gmail.com> wrote:

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Steve Koonin

From: Roy Spencer <roywspencer@hotmail.com>
Sent: Thursday, August 7, 2025 5:30 PM
To: Steven Koonin; 'John Christy'; 'Ross McKittrick'; 'Judith Curry'
Subject: RE: Nature article on economic damage from climate change under review

Wow... if only scientific scandals got legs. Even Climategate was handwaved away. -Roy

Sent from my Verizon, Samsung Galaxy smartphone

----- Original message -----

From: Steven Koonin <steven.koonin@gmail.com>
Date: 8/7/25 4:02 PM (GMT-06:00)
To: 'John Christy' <climateman60@gmail.com>, 'Ross McKittrick' <ross.mckittrick@gmail.com>, 'Judith Curry' <curry.judith@gmail.com>, "'Roy W. Spencer'" <roywspencer@hotmail.com>
Subject: FW: Nature article on economic damage from climate change under review

Ross might already know about this, but it was interesting news to me - SEK

From: Greg Hopper <Greg.Hopper@bpi.com>
Sent: Thursday, August 7, 2025 4:26 PM
To: Steven Koonin <koonin@stanford.edu>
Subject: Nature article on economic damage from climate change under review

Hi Steven,

I really enjoyed reading your recent DOE report (and your book) and in particular the section of the report on the effect of climate change on economic growth.

I don't know if you saw the WaPo article yesterday <https://www.washingtonpost.com/climate-environment/2025/08/06/nature-study-flawed-climate-damages/> that reported on the problems with a paper in Nature <https://www.nature.com/articles/s41586-024-07219-0> from last year that claimed economic damage from climate change is very large. As the WaPo reported, there was a large data error discovered by your Stanford colleague Solomon Hsiang and some co-authors. Hsiang et al, in a "Matters Arising" showed that when the data problem is fixed and uncertainty accounted for, a good bit of the result goes away.

In December last year, I published a research paper at the Bank Policy Institute, where I'm a senior fellow, that raised very serious statistical questions about the Nature model. My criticisms were not high level points easily dismissed. I took a very deep dive into their model, re-estimating and re-simulating it under alternative assumptions. I sent my analysis to the corresponding author of the paper. She never responded. Then I sent the paper to the Nature editors, asking them to look at my criticisms. I offered to do a formal submission according to their process if they wanted. The

editors never answered me. The points I raised are different from Hsiang et al but neither the authors nor Nature responded to them.

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I've attached my analysis from last December FYI.

Greg Hopper

<https://bpi.com/people/greg-hopper/>

From: Ross McKittrick <ross.mckitrick@gmail.com>
Sent: Thursday, August 7, 2025 5:22 PM
To: Steven Koonin
Cc: John Christy; Judith Curry; Roy W. Spencer
Subject: Re: FW: Nature article on economic damage from climate change under review

Yes, I saw the article yesterday. One of our DoE reviewers had asked us to cite the Kotz et al. study and in my response I pointed out that it was subject to some ongoing critiques about data and methodology errors so it was premature to rely on it. The critique just published by Hsiang and coauthors was pretty brutal. Much of the Kotz result came from including erroneous outlier data from a single country (Uzbekistan) and when it was removed the results collapsed. There were other problems as well. The fact that the NGFS people relied on it so uncritically is shameful.

I have a new paper forthcoming in Climate Change Economics with a former PhD student that uses two newly-constructed global data bases and adds to the evidence that warming has only small temporary effects on income (that are + or - depending on the location) and no net impacts on growth.

<https://www.rossmckitrick.com/uploads/4/8/0/8/4808045/cc.and.growth.accepted.version.pdf>

I didn't cite it in CWG25 because it hadn't been accepted yet but if needed it's fair game now. I sent it to Richard Tol who's been criticizing our report on his blog but haven't heard back from him.

On Thu, Aug 7, 2025 at 5:01 PM Steven Koonin <steven.koonin@gmail.com> wrote:

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From: Greg Hopper <Greg.Hopper@bpi.com>
Sent: Thursday, August 7, 2025 4:26 PM
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I've attached my analysis from last December FYI.

Greg Hopper

<https://bpi.com/people/greg-hopper/>

From: Steven Koonin [steven.koonin@gmail.com]
Sent: 8/7/2025 9:01:45 PM
To: 'John Christy' [climateman60@gmail.com]; 'Ross McKittrick' [ross.mckittrick@gmail.com]; 'Judith Curry' [curry.judith@gmail.com]; 'Roy W. Spencer' [roywspencer@hotmail.com]
Subject: FW: Nature article on economic damage from climate change under review
Attachments: The-NGFSs-New-Climate-Damage-Function-A-Flawed-Analysis-With-Massive-Economic-Consequences_Hopper.pdf

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I've attached my analysis from last December FYI.

Greg Hopper
<https://bpi.com/people/greg-hopper/>

The NGFS's New Climate Damage Function: A Flawed Analysis With Massive Economic Consequences

12.09.24



Introduction

Greg Hopper

609.577.9428

Greg.Hopper@bpi.com

The Network of Central Banks and Supervisors For Greening the Financial System (NGFS) is a supranational consortium of central banks and financial regulators; the United States is represented by the Federal Reserve, the OCC, the FDIC and the FHFA. The NGFS develops climate scenarios that are then used by its members to measure the resiliency of the banking system to climate risks. Thus, the Federal Reserve employed NGFS climate scenarios in its 2023 climate scenario exercise for large U.S. banks, and the ECB employed NGFS scenarios in its 2022 climate risk stress test of the Eurosystem balance sheet.

In early November this year, the NGFS updated the “damage function” component of its climate scenario, which projects the effect of rising temperatures and precipitation on global real income. The new damage function was based entirely on an academic paper published this year in the journal *Nature*.

The updated damage function predicts that global real income will be 19 percent lower by 2050 than it would have been with no climate change, regardless of whether current CO2 emission trends improve or worsen. The damage function projects 60 percent lower global real income by 2100 if current CO2 emission trends worsen. Going forward, these extreme economic assumptions in the scenarios would be used in climate stress tests of banks, practically ensuring that those tests will conclude that all global banks face material climate risks that would require higher capital levels.

This paper reviews the new damage function in the *Nature* paper and finds no basis for its projections. The statistical procedure used to justify the new damage model is arbitrary and could easily have produced a damage function that would have predicted much smaller losses of global real income. Those much smaller loss projections should not be taken seriously either. In our assessment of the damage function, we found very limited statistical evidence for any causation between the climate variables and material economic damage and no statistical evidence for the purported influence of the temperature variables that drive almost all the economic damage.

Our findings are consistent with the current state of academic damage function research, which is highly uncertain at this point. One recent academic paper tested 800 plausible specifications of climate damage functions, finding that damages ranged from large economic benefits to large economic losses.¹ With no academic consensus on climate damage functions, it is unclear why the NGFS would adopt the results of a single academic paper, especially given the massive ramifications for global economic growth.

¹ Newell, R, Prest, B, and Sexton, S, “The GDP-Temperature relationship: Implications for climate change damages,” *Journal of Environmental Economics and Management*, (2021)

Why Did the NGFS Update Its Damage Function?

The new damage function is based on academic research by Kotz, Levermann and Wenz², published in Nature in 2024. The adoption of the new model, which is discussed in a companion technical document³, is intended to ensure that the NGFS climate scenarios remain state-of-the-art, incorporating the latest data and methodology. Damage functions are essential components of climate scenarios and models that perform cost-benefit analysis of climate policies, since they suggest how to trade off the costs of climate policy changes against economic losses caused by climate change.

The new model includes 10 distinct types of temperature and precipitation variables and incorporates the influence of climate change in past years on economic damage today, measured at a highly granular geographic level. These new features have given the NGFS confidence that the replacement damage function is much more accurate. In the NGFS release⁴, Sabine Mauderer, Chair of the NGFS and First Deputy Governor of the Deutsche Bundesbank, said, “we have significantly improved the accuracy of physical risk measurements.” Livio Stracca, Chair of the NGFS workstream “Scenario and Design Analysis” and Deputy Director, General Financial Stability at the European Central Bank, suggested the new damage function implies that transition risks are greater than previously supposed and that “the scenarios show that climate change is becoming a first order factor for our economies.”

Background on Damage Function Models

A damage function is a statistical relationship between climate variables and some measure of real output or real income. To estimate such a relationship, researchers gather data on output or income and climate variables such as temperature. One of the original and perhaps the most famous damage functions was formulated by Yale economist William Nordhaus in his DICE model⁵, work for which he shared the Nobel Prize in Economics in 2018. The most recent vintage of the model, completed last year, continues to have a very simple damage function:

$$\%Loss\ in\ global\ real\ GDP = 0.3467\ times\ \Delta T_{mean}^2$$

Thus, if the global mean temperature rises by 3 degrees centigrade (perhaps by 2100), then the percent loss in global GDP would be

$$0.3467\ times\ 3^2 = 0.3467\ times\ 9 = 3.1\%$$

Similarly, if global mean temperature went up 4.5°C, then the loss in global real GDP would be 7%.

Nordhaus notes in the model documentation that estimating a damage function is challenging, since it is necessary to infer the relationship between temperature and GDP over the recent few decades and then

² Kotz, M, Levermann, A, and Wenz, L. “The economic commitment of climate change,” Nature 601, 2024, available at <https://www.nature.com/articles/s41586-024-07219-0>

³ NGFS, “Damage functions, NGFS scenarios, and the economic commitment of climate change: An explanatory note,” November 2024, available at https://www.ngfs.net/sites/default/files/media/2024/11/05/ngfs_scenarios_explanatory_note_on_damage_functions.pdf

⁴ <https://www.ngfs.net/en/communique-de-presse/ngfs-publishes-latest-long-term-climate-macro-financial-scenarios-climate-risks-assessment-2024>

⁵ The most current version of the DICE model and documentation is available at <https://yale.app.box.com/s/whlqcr7gtzdm4nxnrfhvap2hlzebuvmv/folder/196571686525>

extrapolate the relationship far into the future. Rather than using a strict econometric procedure to estimate the damage function, Nordhaus specifies the model by employing a combination of a survey of the academic literature on damage functions, a survey of literature on “tipping points,” which are potential non-linear impacts from climate change, and some judgmental adjustments. Nordhaus calibrates the damage function according to his view of the preponderance of the evidence as of 2023.

A potential limitation of Nordhaus’s approach is that it assumes that global mean temperature is sufficient to reliably infer the effect on GDP. Since climate variables such as temperature are available at highly granular levels, such as by city, county or province, should we not use this additional information? If measures of real GDP could also be obtained locally, then there would be far more data available to estimate the relationship between real GDP and temperature.

The New NGFS Damage Function

Recent damage function papers in the academic literature, including the NGFS damage function, have adopted this strategy. The NGFS damage function was estimated using the DOSE⁶ database, which measures real GDP per capita – real income – for 83 countries and 1,661 subregions over the years 1960 to 2019. Examples of subregions would be states in the U.S. or provinces in China.

Because the DOSE data is highly granular, it is possible to use many more climate variables than just global average temperature to measure the effect of climate change on real income. The NGFS damage function considers a variety of regional temperature and precipitation statistics observed in the regions in which real income is measured: 1) annual mean temperature in the region; 2) annual total precipitation in the region; 3) daily temperature variability over the year; 4) the number of wet days per year and 5) the total precipitation per year from extremely wet days.

The NGFS damage function does not include explicit squared terms as in the Nordhaus model, but it does include interaction terms such as the product of the change of annual mean temperature and the level of annual mean temperature. Multiplying these variables together ensures that the effect of the change in temperature becomes greater as the level of temperature rises. In all there are five interaction terms: 1) the change in temperature times the level of temperature; 2) the change in temperature variability times the level of temperature; 3) the change in precipitation times the level of precipitation; 4) the change in the number of wet days times the level of wet days; and 5) the change in extreme precipitation times the level of precipitation. Overall, there are 10 climate variables in all in the model, listed in Table 1 below.

⁶ Wenz, L, Carr, R, Kogel, N, Kotz, M, and Kalkuhl M, “DOSE-global data set of reported sub-national economic output,” Nature, 2023, available at <https://www.nature.com/articles/s41597-023-02323-8>

Table 1

Dependent Variable	
Annual growth of GDP per capita	
Independent Variable	Explanation
ΔT	Change in annual mean temperature over the year
$\Delta T \cdot T$	Change in annual mean temperature over the year times the level of annual mean temperature that year
ΔT_{var}	Change in the variability of annual mean temperature over the year
$\Delta T_{var} \cdot \bar{T}$	Change in the variability of annual mean temperature over the year times seasonal difference in annual mean temperature that year
ΔP	Change in total precipitation over the year
$\Delta P \cdot P$	Change in total precipitation over the year times the level of precipitation over the year
ΔWD	Change in the number of wet days over the year
$\Delta WD \cdot WD$	Change in the number of wet days over the year times the number of wet days over the year
ΔP_{ext}	Change in extreme precipitation over the year
$\Delta P_{ext} \cdot T$	Change in extreme precipitation over the year times the level of annual mean temperature over the year

The NGFS damage function uses these 10 variables⁷ to explain the annual real income growth for each of the 1,661 sub-regions. Besides allowing any of these 10 variables to affect real income growth in the year they occur, the model allows the terms to have a lagged influence: the temperature-based variables that occurred as much as 10 years ago can affect real income this year. For example, the temperature in Iowa in 1967 has an independent effect on GDP growth in Iowa in 1977 even after including the influence of the temperature-based variables in 1977, 1976, ...1968. The precipitation variables as many as four years ago are allowed to affect real income growth today. The damage function uses three regression models which are combined by switching randomly between the regressions when economic damage is simulated into the future. Temperature terms can affect real income growth as much as eight, nine or 10 years in the future in the three versions of the regression

⁷ The model is a panel regression that includes fixed effects, i.e., constants that differentiate the effect of the climate variables on the sub-regions and over time.

model, with precipitation affecting real income growth as much as four years in the future in each model version.

Overall, as many as 74 climate variables can affect real income growth this year – the four temperature-based variables that occur this year, from last year and from as many as 10 years ago, (4 x 11) and the six precipitation-based variables whose influence is felt this year, from last year and from as many as four years ago (6 x 5).

Relationship of New Damage Function to Old Damage Function

The new damage function is a generalization of the old NGFS damage function, which was based on research by Kalkuhl and Wenz (2020).⁸ In Kalkuhl and Wenz (2020), loss to real income depended on ΔT and $\Delta T \cdot T$. The new damage function includes lags of these variables up to 10 years as well as new temperature variables, ΔT_{var} and $\Delta T_{var} \cdot \bar{T}$, along with lags up to 10 years. Besides these additional temperature variables, the new model also includes the precipitation variables ΔP , $\Delta P \cdot P$, ΔWD , $\Delta WD \cdot WD$, ΔP_{ext} and $\Delta P_{ext} \cdot T$, along with four years of lags. Thus, the new damage model contains dramatically more variables than the old model.

Calibration of the New Damage Function

To calibrate the new damage function, coefficients on each climate variable and its lags are estimated from historical data on real income growth and the climate variables at a high spatial granularity. If we estimate a regression model for one of the three regression models, with eight lags on the temperature variables, we get the regression coefficients in Table 2.⁹

Table 2

Climate Variable	Today	1 year ago	2 years ago	3 years ago	4 years ago	5 years ago	6 years ago	7 years ago	8 years ago
ΔT	0.00051	-0.00524	-0.00617	0.00404	-0.00238	-0.00180	0.01013	0.00323	-0.00047
$\Delta T \cdot T$	-0.00094	-0.00124	-0.00067	-0.00160	-0.00093	-0.00168	-0.00214	-0.00168	-0.00070
ΔT_{var}	0.00613	0.05039	0.00358	-0.04823	0.03171	0.03785	0.02692	0.03162	0.00113
$\Delta T_{var} \cdot \bar{T}$	0.00127	-0.00110	0.00021	-0.00124	-0.00442	-0.00416	-0.00386	-0.00387	-0.00404
ΔP	0.00002	0.00010	0.00008	0.00000	0.00004				
$\Delta P \cdot P$	-4.58E-10	2.63E-08	2.38E-08	-5.67E-08	2.17E-08				
ΔWD	-0.00064	0.00190	-0.00240	-0.00307	0.00158				
$\Delta WD \cdot WD$	3.78E-06	0.00001	0.00001	0.00002	0.00003				
ΔP_{ext}	-0.00025	-0.00028	-0.00039	-0.00029	-0.00009				
$\Delta P_{ext} \cdot T$	0.00001	0.00001	0.00001	0.00001	3.16E-06				

Legend:

	Statistically significant at 5% level
	Statistically significant at 1% level
	Statistically significant at 0.1% level

To calculate an estimate of the loss to real income this year for a particular region, we collect the values of the temperature variables in the region of interest listed in Table 2 for this year and for each year in

⁸ Kalkuhl, M and Wenz, L, "The impact of climate conditions on economic production: evidence from a global panel of regions," Journal of Environmental Economics and Management, (2020), available at <https://www.sciencedirect.com/science/article/pii/S0095069620300838>

⁹ Table 2, which we have re-estimated, corresponds to Supplementary Table 2 in Kotz, M., Levermann A, and Wenz, L, "Supplementary information for: The economic commitment of climate change," 2024, available at https://static-content.springer.com/esm/art%3A10.1038%2Fs41586-024-07219-0/MediaObjects/41586_2024_7219_MOESM1_ESM.pdf

the past up to eight years ago. We would also collect the values of the precipitation variables this year and up to four years ago. We then take each climate variable, multiply it by its corresponding coefficient in Table 2 and then add. For example, we would take the change in temperature this year and multiply it by 0.00051. To this product, we would add the change in temperature last year multiplied by -0.00524. We continue this process until we have added up all 66 terms. That gives us the damage this year. Then we move to next year and repeat the process.

As we move forward in time, we will have to rely on climate models to give us projections of temperature and precipitation variables to use in calculations. We would repeat the process for each year from 2020 to 2080, giving a cumulative damage function for real income in the region we are focusing on. Then we repeat the process for all regions to get the effect on specific countries and globally. Because each year's damage accumulates the effects of climate change in previous years, the damage compounds to large numbers. Oddly, not only does climate change up to 10 years in the past have independent effects on GDP today, as can be seen in Table 2, losses from climate change further in the past can have a bigger effect on real income today than climate change in more recent years.

Although the procedure outlined above is conceptually how damages are calculated, the model is more complicated. The data set is shuffled randomly one thousand times and then the model is re-estimated 1,000 times, analogous to drawing a different hand from the shuffled deck. In the end, we have 1,000 versions of Table 2.¹⁰ After drawing a set of regression coefficients randomly, the Kotz et al (2024) procedure is to randomly select a future path of climate variables from a set of climate models that project climate variables into the future from 2020 to 2100. The future paths of the climate are selected for two scenarios: RCP 2.6 and RCP 8.5. RCP 2.5 is a scenario in which temperature rises until about 2050 and then levels off. RCP 8.5 is a scenario in which GHG emissions get much worse over time, implying a very high rise in temperature well over 3°C. Repeating these simulations¹¹, we get the following results¹².

¹⁰ The same procedure is followed to estimate 1,000 regressions with nine lags on the temperature variables and 1,000 regressions with 10 lags on the precipitation variables. In the model simulations, regression coefficients are drawn randomly from one of the three models.

¹¹ We use the software and data provided by Kotz, Levermann, and Wenz (2024), available at <https://zenodo.org/records/13957801>

¹² This chart is a re-simulated version of Figure 1 in Kotz et al (2024).

Figure 1

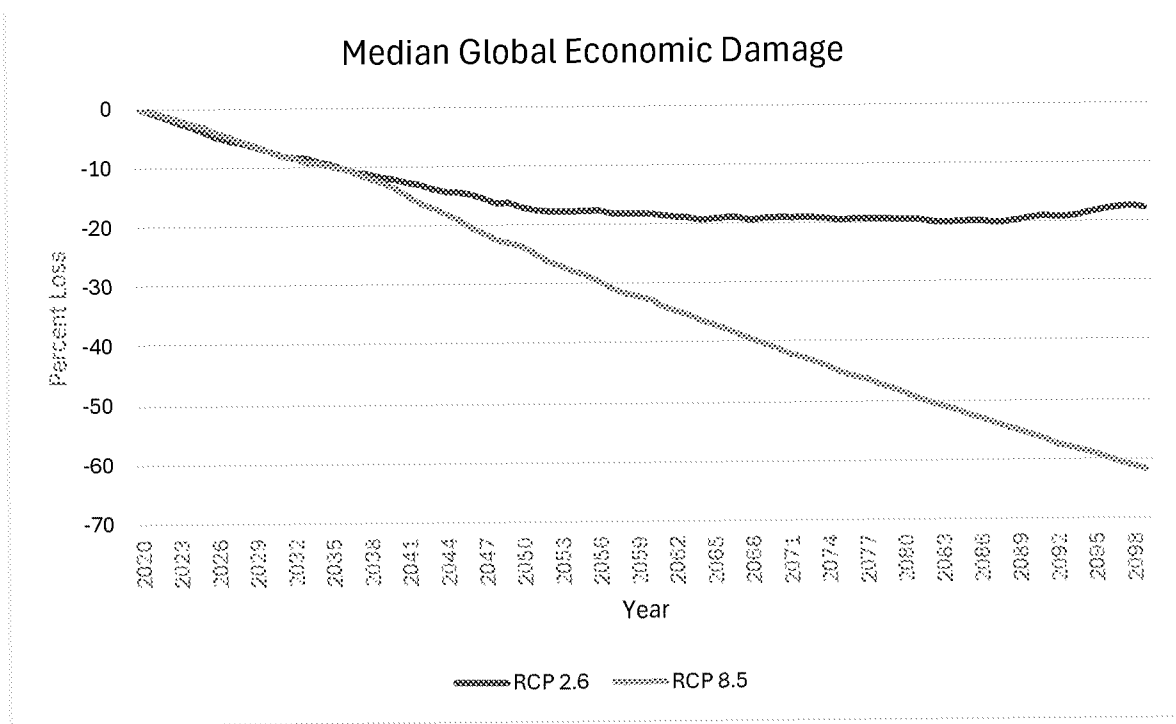
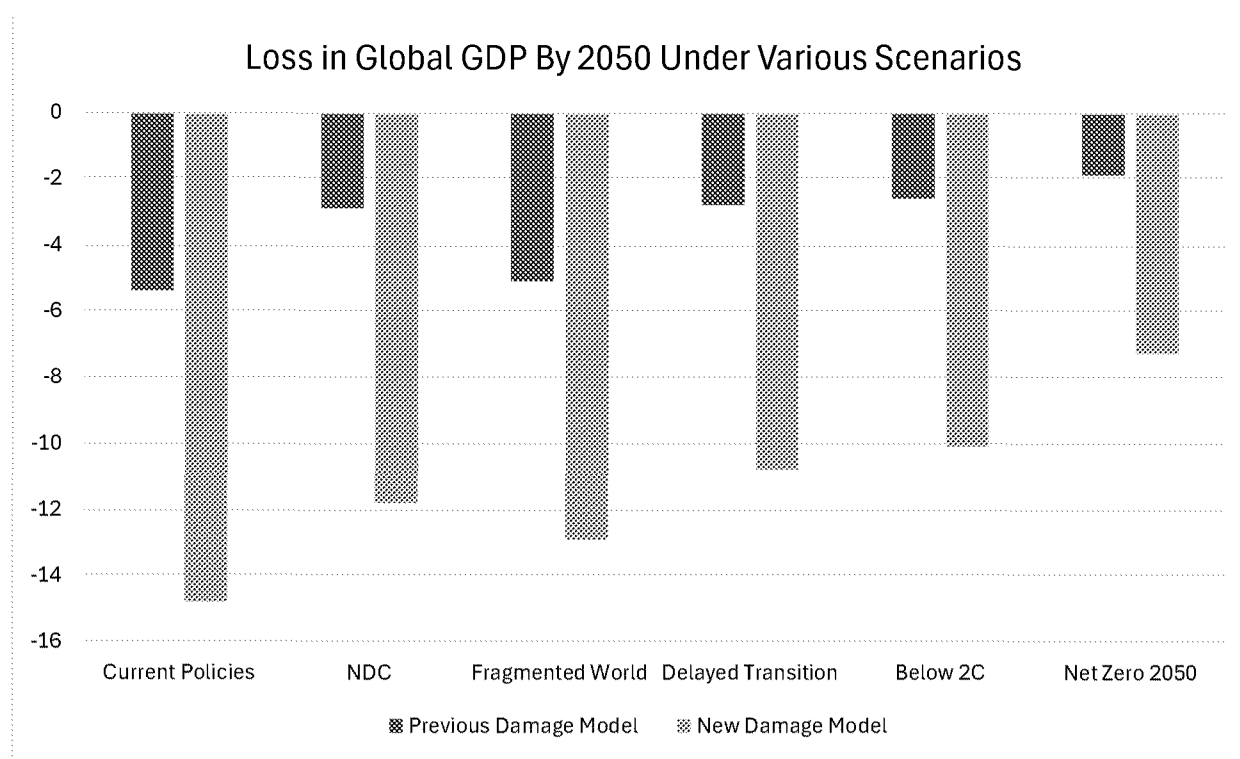


Figure 1 shows that global economic damage is fairly similar up to about 2050 under the two scenarios, with global damage leveling off at about 20 percent under RCP 2.6 but continuing to about 60 percent under RCP 8.5. The model simulations show that -19 percent is the “committed” economic damage under the model, i.e., considering the damage under both RCP 2.6 and RCP 8.5, -19 percent is the point at which the damage implied by the two scenarios can be statistically distinguished. A damage of -19 percent is inevitable regardless of the climate policy the world follows, according to the model. Figure 2 shows the difference between the old and new NGFS damage functions for various NGFS scenarios.¹³

¹³ The Kotz et al damage function cannot be directly applied to the NGFS scenarios since it contains extra climate variables. It is ported to a function that depends on temperature and locations to be used within the NGFS scenarios.

Figure 2



Is the New Damage Function Really Better?

In its technical document, the NGFS offers some purported key advantages of the new damage function relative to the old damage function. The NGFS argues that since the new damage function is based on a variety of temperature and precipitation variables measured at a high geographic granularity, it should be more accurate. Moreover, the NGFS maintains that the inclusion of lags up to 10 years can capture the potential effects of climate change on real income that can persist for many years, an effect ignored by the old damage function.

Of course, whether including new climate variables in the new damage function is important depends on whether they practically matter in the damage projections. In Annex 4 of its technical document, the NGFS claims the additional variables contribute materially to the damage projections.¹⁴ The NGFS analysis attempts to allocate the relative contributions of the climate variables to the global loss of 61.6 percent of real income produced by the new damage function under RCP 8.5 by 2100. The results of the NGFS analysis are shown in Table 3. According to the NGFS analysis, all of the variables in the model make material contributions to the final damage estimate, justifying the use of the new climate variables in the model.

¹⁴ See NGFS technical document, page 49

Table 3

Climate Driver	%points
Instantaneous impact of mean temperature	14.2
Persistence effects mean temperature	28.9
Temperature variability	16
Precipitation	2.5
Total damages	61.6

To check the NGFS estimates, we follow a different strategy. We can verify how much each set of variables matters by zeroing out coefficients of different categories of climate variables in the underlying regression models and then re-simulating the NGFS damage model. If we look at Table 4, in which we have circled the estimates for the second row, we can see that the $\Delta T \cdot T$ variables in the regression are good candidates for being highly influential. All the estimates are negative, the coefficient values are relatively large compared to the other coefficients and they are multiplied by the absolute level of temperature that is scaled by the change in temperature.

Table 4

Climate Variable	Today	1 year ago	2 years ago	3 years ago	4 years ago	5 years ago	6 years ago	7 years ago	8 years ago
ΔT	0.00051	-0.00524	-0.00617	-0.00404	-0.00238	-0.00180	-0.01013	-0.00323	-0.00847
$\Delta T \cdot T$	-0.00094	-0.00124	-0.00067	-0.00160	-0.00093	-0.00153	-0.00214	-0.00193	-0.00070
ΔT_{var}	-0.00011	-0.00039	-0.00056	-0.04823	0.03171	0.03785	0.02692	0.03162	0.00113
$\Delta T_{var} \cdot T$	0.00127	-0.00110	0.00021	-0.00124	-0.00447	-0.00416	-0.00354	-0.00367	-0.00404
ΔP	0.00002	0.00010	0.00009	0.00003	0.00004				
$\Delta P \cdot P$	-4.58E-10	-2.63E-08	-2.38E-08	-1.07E-06	-2.17E-06				
ΔWD	-0.00064	0.00190	0.00240	-0.00302	0.00153				
$\Delta WD \cdot WD$	3.78E-06	0.00001	0.00001	0.00002	0.00001				
ΔP_{ext}	-0.00025	-0.00023	0.00030	-0.00029	-0.00009				
$\Delta P_{ext} \cdot T$	0.00001	0.00001	0.00001	0.00001	3.16E-06				

Legend:

	Statistically significant at 5% level
	Statistically significant at 1% level
	Statistically significant at 0.1% level

To test the effect of that variable in the model, we take the 1,000 simulated regressions without re-estimating them and zero out all coefficients other than the nine coefficients in the second row of Table 4. We then re-simulate the NGFS damage model along the RCP 2.6 and 8.5 scenarios, drawing randomly from the same set of climate models used to determine the NGFS damage function.¹⁵ In this way, only the coefficients in the second row of Table 4 contribute to the projected damage. Figure 3 shows the result.

¹⁵ We also do the same for the thousand regressions with nine and 10 temperature variables and include these regressions in the simulation.

Figure 3

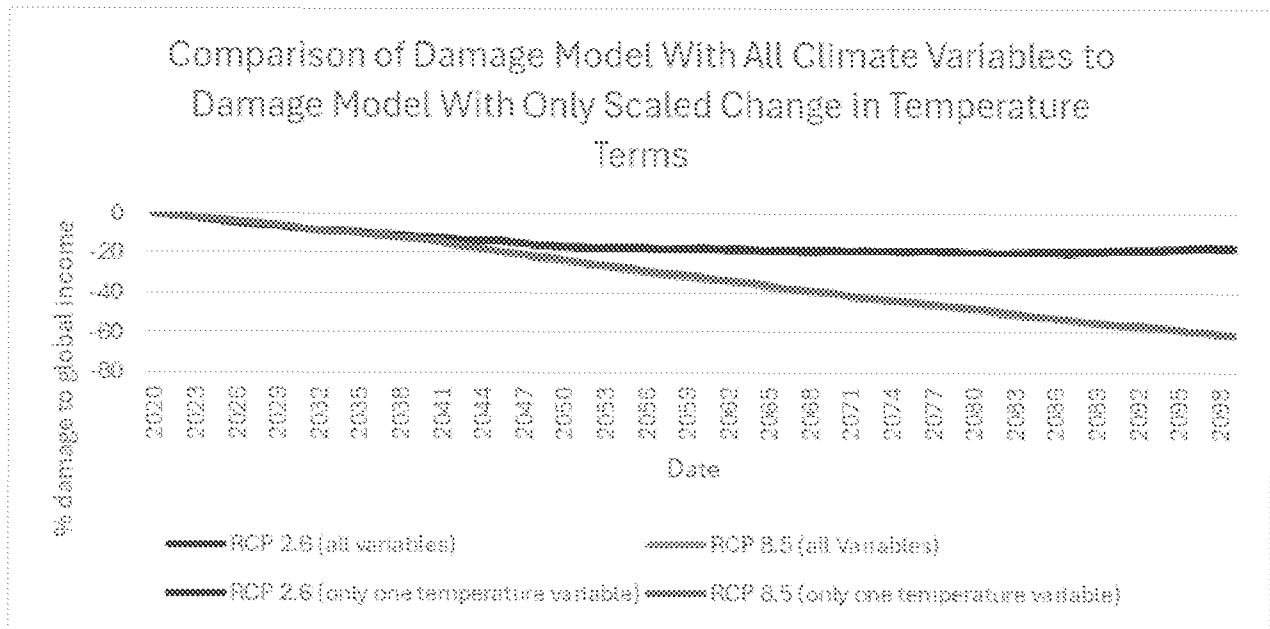
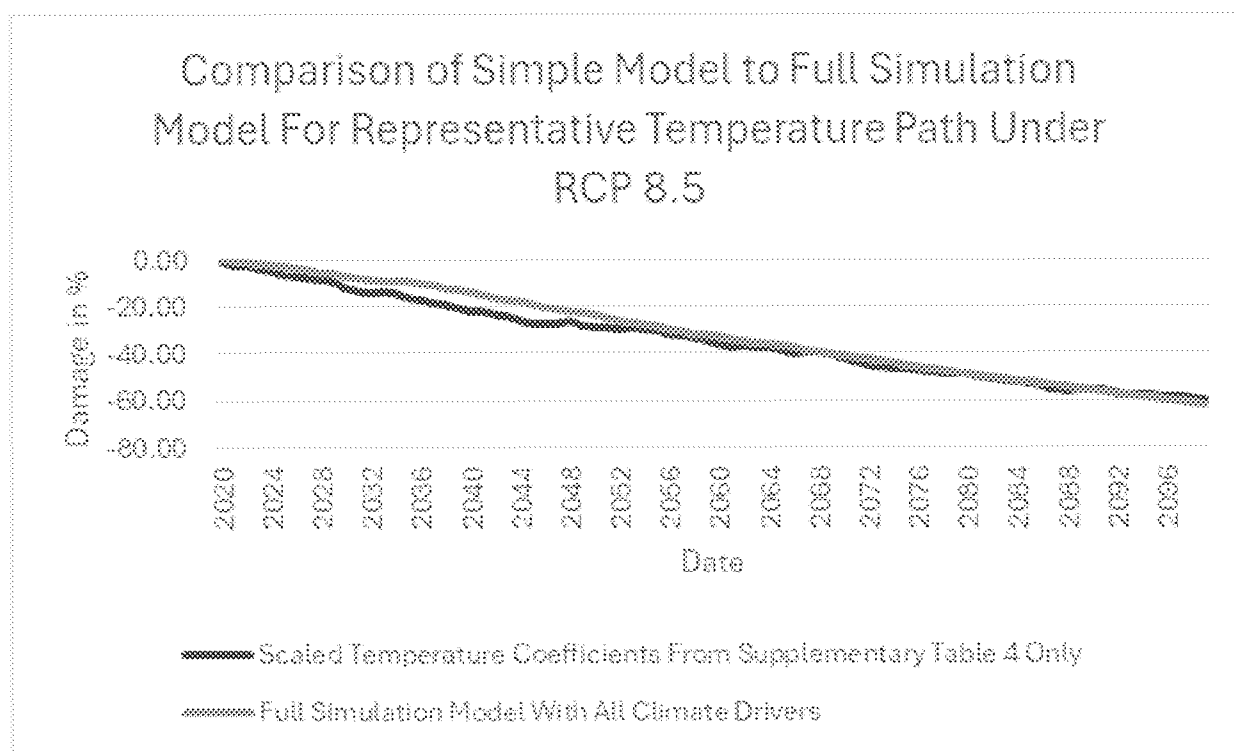


Figure 3 demonstrates that a damage model that only includes the scaled level of temperature terms from the regressions closely follows the full NGFS model that includes all the climate variables. In particular, the model simulations using all the climate variables estimated a global real income loss under RCP 8.5 by 2100 of 61.7 percent while the model simulations with only the scaled level of temperature variables produced a loss of 61 percent. This test contradicts the NGFS analysis that the newly added climate variables contribute significantly to the results.

As another check, we can pull out a temperature increase scenario under RCP 8.5 under one of the climate projection models¹⁶ used by Katz et al (2024) to estimate the damage to 2100. In this experiment, for each year we predict the economic damage by using only the scaled level of temperature coefficients from Supplementary Table 4 in the Supplementary information in Katz et al (2024), i.e., we only include the current and lagged values of $\Delta T \cdot T$. Figure 4 demonstrates that the simplified regression model with only the temperature terms predicts the full simulation outcome well.

¹⁶ We use the global circulation model ACCESS-ESM1-5 for this test

Figure 4



Although the NGFS touts the inclusion of a wide variety of temperature and precipitation variables as a key advantage of the new damage model, a full model simulation as well as a simplified check suggests that the newly included variables are irrelevant in practice. The damage function primarily depends on the level and change of temperature. Adding the additional climate variables does not matter practically.

How Much Does Each Lag of the Scaled Level of Temperature Matter?

Although the large number of lags of the one variable, the scaled level of temperature $\Delta T \cdot T$, explains the large damage estimates, it is not clear how much each lag contributes to the total. To answer this question, we zero out all coefficients reported in Table 4 other than the second row.¹⁷ We then fully re-simulate the damage model, using the assumption of no lags, one lag, two lags, three lags, five lags and seven lags of $\Delta T \cdot T$. Figure 5 shows the results when either seven or five lags of $\Delta T \cdot T$ only are kept in the model.

¹⁷ We follow the same procedure for the other two regression models used in the damage function.

Figure 5

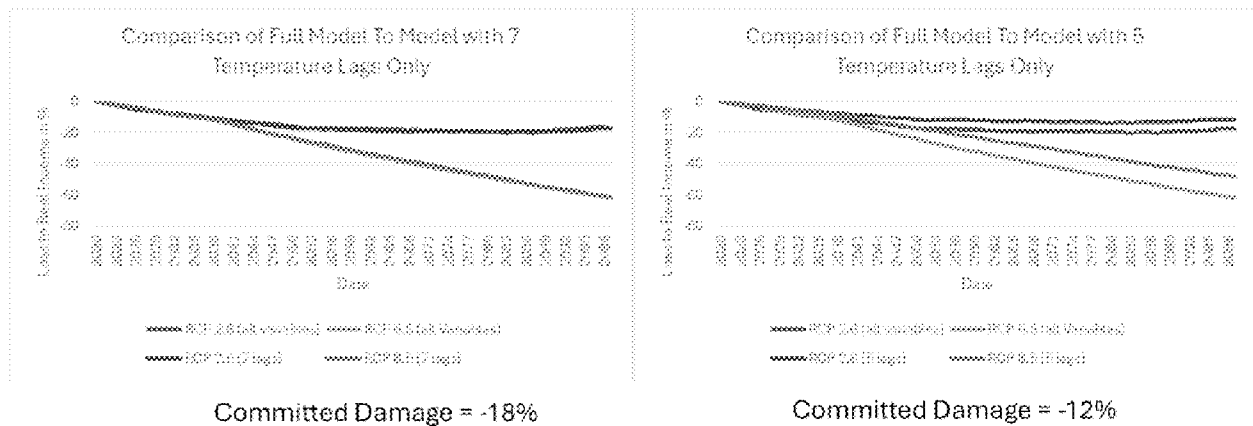
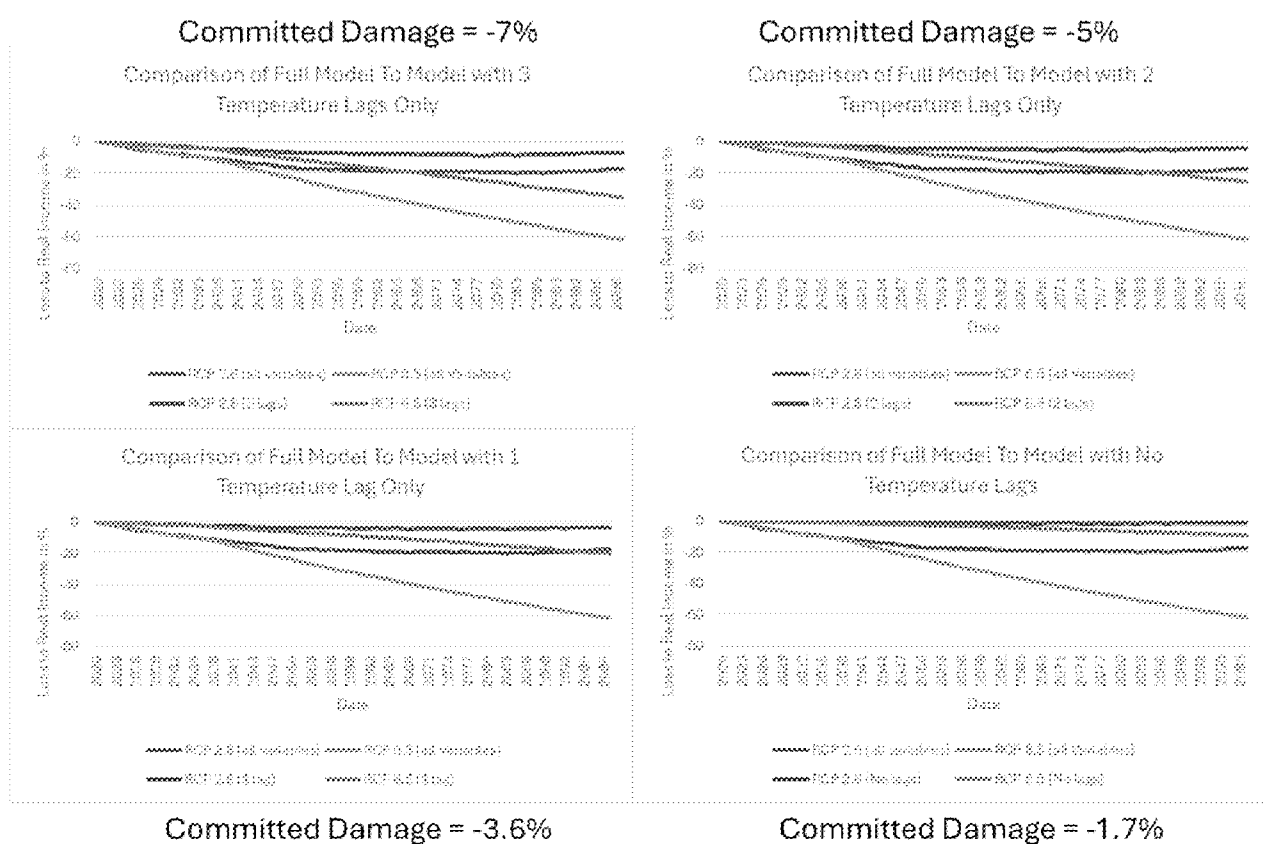


Figure 5 shows that seven lags in the temperature variables are sufficient to reproduce the full damage function. However, we start to see significantly lower damage with five lags.

Figure 6 shows the effect of including three, two, one or no lags in the model.

Figure 6



Starting with no lags of the $\Delta T \cdot T$ temperature variables, Figures 5 and 6 show that the addition of each lag contributes significantly to the economic damage predicted by the model, with the effect of the lags saturating at about seven lags. The coefficients of the lags do not decline with time, but sometimes are greater than the contemporaneous effect. Adding up many lags, all of which are of the same order of magnitude, produces large economic losses. Thus, the statistical justification for including each lag of the scaled temperature in the model is vitally important if we are to believe the large economic damage results.

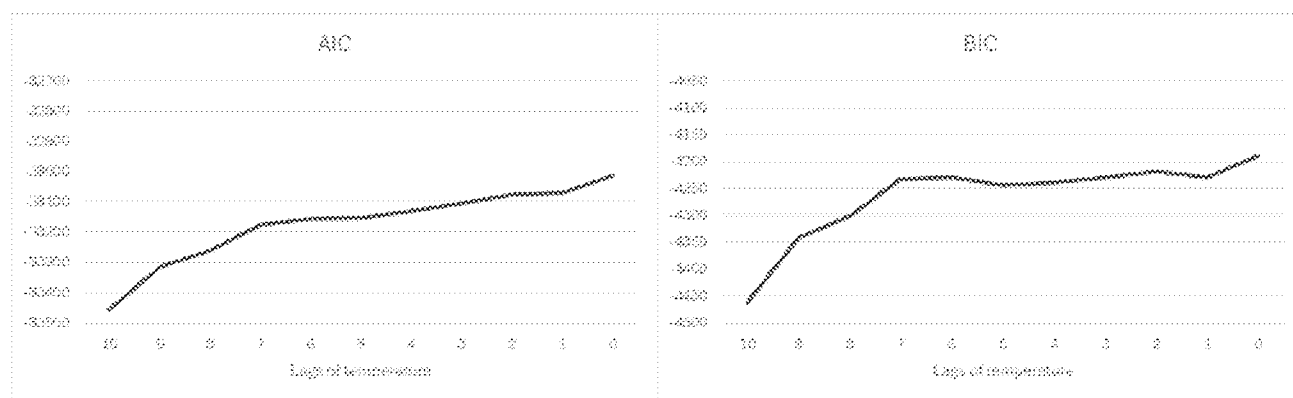
Is the Damage Model Statistically Justified?

Model Selection Criteria are Arbitrary

Kotz et al (2024) justify the included variables and their lags by using a combination of the Akaike Information Criterion (AIC) and related Bayesian Information Criterion (BIC).¹⁸ The AIC and BIC criteria are goodness-of-fit measures that attempt to trade off goodness of fit against overfitting, i.e., including too many variables and lags in the regression model. The BIC penalizes the inclusion of additional variables more severely and is a more conservative measure.¹⁹ Lower AIC and BIC measures are better. Kotz et al (2024) determine the number of lags to keep in the regressions by selecting models that minimize the AIC and BIC.

Kotz et al (2024) start with a model with 10 lags of temperature and 10 lags of precipitation variables and then remove one lag of one variable at a time. For example, Figure 7 plots AIC and BIC starting with 10 lags of all the variables and removing one lag of the change in temperature variables at a time, holding the lags of all other variables constant. Figure 7 shows that keeping all 10 lags of the change in temperature variable results in the minimum AIC and BIC, conditional on keeping all other variables at 10 lags. Figure 8 repeats the same procedure for the change in precipitation variable, suggesting that four lags of precipitation minimize the AIC and BIC.

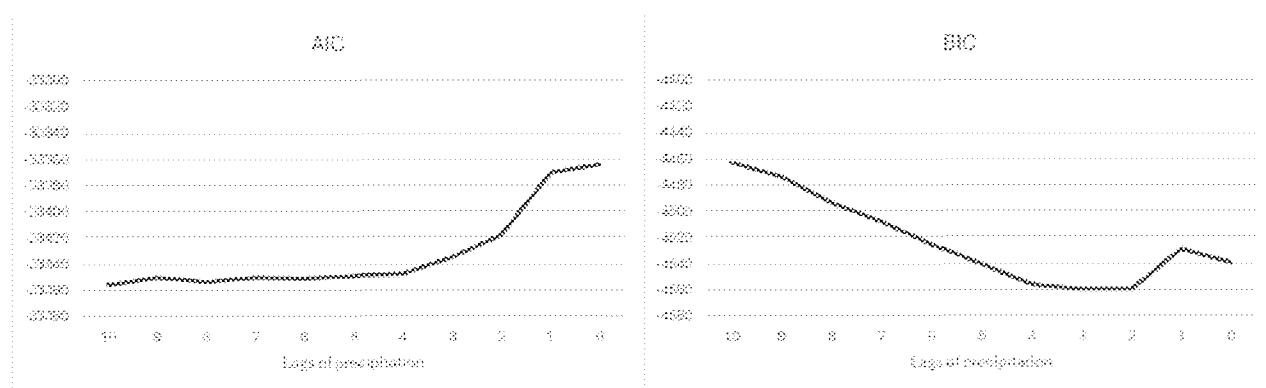
Figure 7



¹⁸ If $\hat{\theta}$ is the estimated parameter vector of k variables, and $\ln L(\hat{\theta})$ is the log-likelihood function, then $AIC = -2\ln L(\hat{\theta}) + 2k$. Similarly, $BIC = -2\ln L(\hat{\theta}) + k\ln(n)$, where n is the number of data points.

¹⁹ The $\ln(n)$ term makes the BIC larger, especially for regressions that have a lot of data points n .

Figure 8



Continuing with this lag selection methodology, Kotz et al (2024) find that models with four lags of precipitation variables and either eight, nine or 10 lags of temperature variables minimize the AIC and BIC²⁰. They end up with three preferred specifications of regression model. Table 5 shows the AIC and BIC for each model.

Table 5

	10 Temperature Lags and 4 Precipitation Lags	9 Temperature Lags and 4 Precipitation Lags	8 Temperature Lags and 4 Precipitation Lags
AIC	-3.34E+04	-3.38E+04	-3.42E+04
BIC	-4.68E+03	-5.06E+03	-5.49E+03

The model selection procedure employed by Kotz et al (2024) is arbitrary. Why start with 10 lags of all variables and then remove one variable at a time, holding the lags of the others constant? There are many other ways to proceed. For example, if limiting overfitting is the concern, why not start with the simplest possible model and then add lags? Suppose we start with a candidate model with no lags and then we add them one by one, choosing the model that minimizes the AIC and BIC. Table 6 shows the result of applying this alternative procedure.

²⁰ The results of the AIC and BIC tests explains why Kotz et al use three regressions with eight, nine or 10 temperature variable lags and four lags of the precipitation variables, randomly switching between the three regressions when they simulate economic damage from the model.

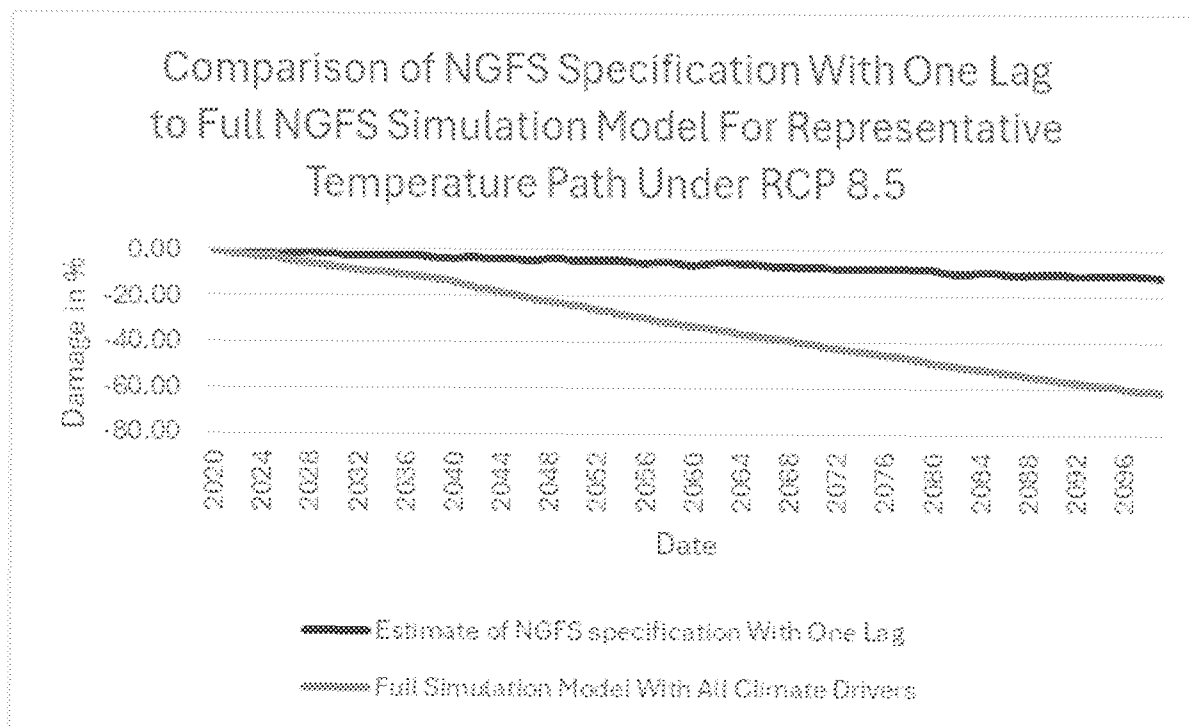
Table 6

	10 Temperature Lags and 4 Precipitation Lags	9 Temperature Lags and 4 Precipitation Lags	8 Temperature Lags and 4 Precipitation Lags	No Temperature and Precipitation Lags	1 Temperature and Precipitation Lag	2 Temperature and Precipitation Lags
AIC	-3.34E+04	-3.38E+04	-3.42E+04	-3.64E+04	-3.66E+04	-3.63E+04
BIC	-4.68E+04	-5.06E+04	-5.49E+03	-7.77E+03	-7.94E+03	-7.59E+03

The damage model with one lag of all temperature and precipitation variables has the lowest AIC and BIC statistics relative to the alternative models with no lags and two lags. More importantly, the model with one lag would be preferable to any of the NGFS damage model specifications in the first three columns of Table 6, since the AIC and BIC statistics are lower.

To benchmark the difference in economic damage implied by the one-lag damage model and the NGFS damage model, we run the temperature terms of the one-lag model on the representative RCP 8.5 scenario used in Figure 4. Figure 9 shows, not surprisingly, that the one-lag model produces 80 percent lower economic damage by 2100 compared to the NGFS damage model.

Figure 9



The Model Selection Process Did Not and Cannot Establish Statistical Significance of the Underlying Parameters

Kotz et al (2024) as well as the NGFS technical document emphasize that the goal of the damage model is to identify “robustly inferred causal relationships” that can be used “to project the exogenous impact of future climate conditions on the economy.”²¹ However, the model selection procedure used to specify the details of the NGFS damage model is not relevant for establishing causation.

Kotz et al (2024) and the NGFS technical document imply that the use of the AIC and BIC helps to establish causation, although they do not spell out the argument as to how this would work. To infer causal relationships, it is necessary to estimate the magnitudes of the effects of climate variables on real income growth, ensuring that they are statistically significant. Kotz et al (2024) and the NGFS technical document do not make the explicit claim that the AIC and BIC analysis establishes causation between climate variables and economic damage by showing that the underlying estimates are statistically significant. But if they did make such an explicit claim, it would be invalid.

The AIC and BIC can be used to establish statistical significance of the parameters of the damage model, but only under the highly unrealistic assumption that the unobserved influences in the model are uncorrelated.²² The methodology of restricting variables and preferring a model with a lower AIC or BIC is equivalent to running a hypotheses test, specifically a likelihood ratio test.²³ For the likelihood ratio test to be a valid statistical test, the unobserved influences in the panel regression model must be uncorrelated.

As emphasized by Cameron and Miller²⁴, panel data regression models, of which the NGFS damage model is an example, have a pronounced tendency to overstate statistical significance of variables if the correlation across the cross-sectional data—the subregions—and correlation across time—years—is not accounted for, leading to spurious statistical significance. In general, statistical hypothesis tests should be corrected for correlation, but the AIC and BIC are uncorrected. Since the underlying unobserved influences in the damage model are almost certainly correlated across subregions and across time, the AIC and BIC cannot be interpreted as statistical hypothesis tests that help to establish statistical significance, and by implication, causation. The AIC and BIC can only be used to make relative comparisons in sample between different model specifications, balancing goodness of fit against overfitting.

²¹ Kotz et al, Supplementary Information, pg 5. Also see NGFS Technical Document, pg 19

²² In econometric terms, the error terms in the model must be homoscedastic and serially uncorrelated.

²³ If we have two nested models M_1 and M_2 with $M_1 \subset M_2$ with log-likelihoods $\ln L_1(\hat{\theta}_1)$ and $\ln L_2(\hat{\theta}_2)$ and $k_1 < k_2$ parameters, then we would select M_1 if $AIC(M_1) < AIC(M_2)$ or $-2\ln L_1(\hat{\theta}_1) + 2k_1 < -2\ln L_2(\hat{\theta}_2) + 2k_2$, which is a standard likelihood ratio test since $LR = 2(\ln L_2(\hat{\theta}_2) - \ln L_1(\hat{\theta}_1)) < k_2 - k_1$, distributed χ^2 . Thus, when we select models that have lower AICs, we are implicitly performing likelihood ratio tests with a critical value of $k_2 - k_1$. Similarly, when we select models with lower BICs, we are implicitly performing likelihood ratio tests with a critical value of $(k_2 - k_1)\ln(n)$. Likelihood ratio tests are alternatives to the standard F and t tests.

²⁴ Cameron, A and Miller, D, “A Practitioner’s Guide to Cluster-Robust Inference,” Journal of Human Resources, 2015, available at <https://jhr.uwpress.org/content/50/2/317>

Important Coefficients Are Not Statistically Significant

Strangely, despite the emphasis on causation, Kotz et al (2024) do not focus on the statistical significance of the estimated parameters. Table 2, which corresponds to Supplementary Table 2 in Kotz et al (2024), does report statistical significance of the parameters. All but one of the coefficients in the second row, the variable that produces almost all the economic damage, are apparently statistically significant. However, these statistics are misleading. The reported standard errors²⁵ in Kotz et al (2024) are clustered by region, which assumes there are correlations of unobserved factors that affect regions²⁶. However, the unobserved influences in the model are almost certainly also correlated across time, which is why it is common practice to cluster standard errors in the time dimension as well, i.e., double cluster the standard errors.

Recent research papers on similar panel regression-based climate damage functions have recognized that unobserved influences are correlated cross-sectionally and across time and have reported double clustered standard errors. Examples include Newell et al (2021), Burke et al (2015)²⁷ and Dell et al (2012).²⁸ In other climate-related econometric research that features panel regressions, Bolton and Kacperczyk (2023)²⁹ report their estimates of the existence and magnitude of a carbon premium in the equity markets after double clustering standard errors along the cross section and time dimensions. Similarly, Hopper (2024)³⁰ estimates a panel regression model that measures the effect of GHG emissions on probability of default and double clusters the standard errors across cross-sections and time. Kotz et al (2024) did not follow this best practice.

Table 7 reports the statistical significance levels that would have been obtained by double clustering the standard errors of the NGFS damage function regression model, compared to the singly clustered standard errors by region reported in Kotz et al (2024). When we single cluster or double cluster the standard errors, we do not change the estimates themselves. We only change the standard errors, which changes the statistical significance levels.

²⁵ We did not report the standard errors explicitly in Table 2 but rather color-coded what the standard errors imply for statistical significance.

²⁶ Supplementary tables 2, 3 and 4 in Supplementary Information, Kotz et al (2024), report standard errors clustered at the region level only.

²⁷ Burke, M and Hsiang, S, "Global non-linear effects of temperature on economic production," *Nature*, (2015)

²⁸ Dell, M, Jones, B, and Olken, B, "Temperature shocks and economic growth: evidence from the last century," *American Economic Journal of Macroeconomics*, (2012)

²⁹ Bolton, P and Kacperczyk, M, "Global Pricing of Carbon Transition Risk," *Journal of Finance*, August 2023, available at <https://onlinelibrary.wiley.com/doi/10.1111/jofi.13272>

³⁰ Hopper, G, "How Should Banks Manage Climate Transition Risk?" (2024), available at <https://bpi.com/how-should-banks-manage-climate-transition-risk/>

Table 7

Clustered By Region									
Climate Variable	Today	1 year ago	2 years ago	3 years ago	4 years ago	5 years ago	6 years ago	7 years ago	8 years ago
ΔT	0.00051	-0.00524	-0.00617	0.00404	-0.00238	-0.00180	0.01013	0.00323	-0.00847
$\Delta T \cdot T$	-0.00094	-0.00124	-0.00067	-0.00160	-0.00093	-0.00188	-0.00214	-0.00168	-0.00070
ΔT_{var}	-0.08611	-0.05039	-0.09350	-0.04823	0.03171	0.03785	0.02692	0.03162	0.09113
$\Delta T_{var} \cdot \bar{T}$	0.00127	-0.00110	0.00021	-0.00124	-0.00442	-0.00416	-0.00358	-0.00367	-0.00404
ΔP	0.00002	0.00010	0.00009	0.00008	0.00004				
$\Delta P \cdot P$	-4.58E-10	-2.63E-08	-2.38E-08	-3.07E-08	-2.17E-08				
ΔWD	-0.00064	-0.00190	-0.00240	-0.00302	-0.00155				
$\Delta WD \cdot WD$	3.78E-06	0.00001	0.00001	0.00002	0.00001				
ΔP_{ext}	-0.00025	-0.00028	-0.00030	-0.00029	-0.00009				
$\Delta P_{ext} \cdot T$	0.00001	0.00001	0.00001	0.00001	3.16E-06				
Double Clustered Standard Errors									
Climate Variable	Today	1 year ago	2 years ago	3 years ago	4 years ago	5 years ago	6 years ago	7 years ago	8 years ago
ΔT	0.00051	-0.00524	-0.00617	0.00404	-0.00238	-0.00180	0.01013	0.00323	-0.00847
$\Delta T \cdot T$	-0.00094	-0.00124	-0.00067	-0.00160	-0.00093	-0.00188	-0.00214	-0.00168	-0.00070
ΔT_{var}	-0.08611	-0.05039	-0.09350	-0.04823	0.03171	0.03785	0.02692	0.03162	0.09113
$\Delta T_{var} \cdot \bar{T}$	0.00127	-0.00110	0.00021	-0.00124	-0.00442	-0.00416	-0.00358	-0.00367	-0.00404
ΔP	0.00002	0.00010	0.00009	0.00008	0.00004				
$\Delta P \cdot P$	-4.58E-10	-2.63E-08	-2.38E-08	-3.07E-08	-2.17E-08				
ΔWD	-0.00064	-0.00190	-0.00240	-0.00302	-0.00155				
$\Delta WD \cdot WD$	3.78E-06	0.00001	0.00001	0.00002	0.00001				
ΔP_{ext}	-0.00025	-0.00028	-0.00030	-0.00029	-0.00009				
$\Delta P_{ext} \cdot T$	0.00001	0.00001	0.00001	0.00001	3.16E-06				

Legend:

	Statistically significant at 5% level
	Statistically significant at 1% level
	Statistically significant at 0.1% level

As Table 7 shows, when accounting for correlation of unobserved influences across regions and time, a good bit of the statistical significance disappears. More concerning, the statistical significance of the terms that drive the most important coefficients on $\Delta T \cdot T$ has vanished,³¹ raising serious questions about the legitimacy of the damage function. Without robust statistical evidence on the causative effect of the $\Delta T \cdot T$ coefficients, the large economic damage from the model disappears.

Conclusions

The academic literature on damage functions is much too uncertain to choose a new damage function that is obviously superior to the many alternatives. Instead, any new damage model should be specified as a hypothetical scenario assumption rather than as an established empirical fact. The NGFS could use a range of damage function models to incorporate different macroeconomic assumptions into its climate scenarios, as long as they are well-tested and plausible. To help ensure that the range of models

³¹ Double clustering the standard errors in the nine- and 10-lag regression models reported in Supplementary Tables 3 and 4 in Kotz et al (2024) produces similar results.

selected is credible, the NGFS should employ an open process with extensive NGFS member review that is disclosed and subject to public comment before any new damage models are officially adopted.

From: John Christy <climateman60@gmail.com>
Sent: Sunday, August 3, 2025 3:43 PM
To: Steve Koonin
Cc: Roy Spencer; Judith Curry; Ross McKittrick; Travis Fisher; Charles Park; Joshua Loucks; Seth Cohen; Audrey Barrios
Subject: Re: Hausfather Santer

Indeed - that's why models do fairly well at his elevations - simple radiative transfer of gasses with well-characterized responses.

John C.

John Christy
Director, Earth System Science Center
Professor, Atmos and Earth Science
Alabama State Climatologist
The University of Alabama in Huntsville
climateman60@gmail.com

On Aug 3, 2025, at 2:28 PM, Steven Koonin <steven.koonin@gmail.com> wrote:

Radiative changes in the stratosphere are a no brainer, I'd think- "just physics" that can be understood with simple 1d transfer. So you'd be surprised if you didn't find them.

Real action is lower down, with that messy convection and water vapor. Not so simple and so a real test of understanding.

Steven E. Koonin

On Aug 3, 2025, at 11:00, Roy Spencer <roywspencer@hotmail.com> wrote:

Again I will suggest NOT to accept the premise that just because more CO2 cools the stratosphere, even if there was perfect agreement with models, tropospheric feedbacks are so uncertain we don't have any confidence in tropospheric and surface warming. Two different issues. - Roy

Sent from my Verizon, Samsung Galaxy smartphone

----- Original message -----

From: John Christy <climateman60@gmail.com>
Date: 8/3/25 8:25 AM (GMT-06:00)
To: Judith Curry <curry.judith@gmail.com>
Cc: Ross McKittrick <ross.mckittrick@gmail.com>, Steve Koonin
<steven.koonin@gmail.com>, Travis Fisher <travis.fisher@hq.doe.gov>, Roy
Spencer <roywspencer@hotmail.com>, Charles Park
<charles.park@hq.doe.gov>, Joshua Loucks <Joshua.Loucks@hq.doe.gov>, Seth
Cohen <seth.cohen@hq.doe.gov>, Audrey Barrios <audrey.barrios@hq.doe.gov>
Subject: Re: Hausfather Santer

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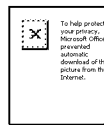
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<https://www.theclimatebrink.com/p/how-the-doe-and-epa-used-and-misused>



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CFAN - Climate Forecast Applications Network
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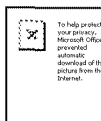
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Subject: RE: Hausfather Santer

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From: John Christy <climateman60@gmail.com>
Sent: Sunday, August 3, 2025 10:05 AM
To: Judith Curry
Cc: Roy Spencer; Ross McKittrick; Steve Koonin; Travis Fisher; Charles Park; Joshua Loucks; Seth Cohen; Audrey Barrios
Subject: Re: Hausfather Santer

I agree. There are a lot of knee-jerk reactions, but when actually addressing the content - not much to complain about.

John C.

Sent from my iPhone

On Aug 3, 2025, at 8:58 AM, Judith Curry <curry.judith@gmail.com> wrote:

After much ranting on the Tropical Listserv, I have gotten a few references that we can add to the report.

Once people actually try to critique this, they will have a tough time I suspect

On Sun, Aug 3, 2025 at 6:56 AM Roy Spencer <roywspencer@hotmail.com> wrote:
That's good. :-)

Sent from my Verizon, Samsung Galaxy smartphone

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Date: 8/3/25 8:55 AM (GMT-06:00)
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Cc: Ross McKittrick <ross.mckittrick@gmail.com>, Steve Koonin <steven.koonin@gmail.com>, Travis Fisher <travis.fisher@hq.doe.gov>, Roy Spencer <roywspencer@hotmail.com>, Charles Park <charles.park@hq.doe.gov>, Joshua Loucks <Joshua.Loucks@hq.doe.gov>, Seth Cohen <seth.cohen@hq.doe.gov>, Audrey Barrios <audrey.barrios@hq.doe.gov>
Subject: Re: Hausfather Santer

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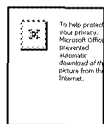
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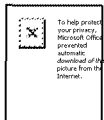
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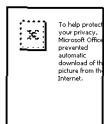
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From: Judith Curry <curry.judith@gmail.com>
Sent: Sunday, August 3, 2025 9:59 AM
To: Roy Spencer
Cc: John Christy; Ross McKittrick; Steve Koonin; Travis Fisher; Charles Park; Joshua Loucks; Seth Cohen; Audrey Barrios
Subject: Re: Hausfather Santer

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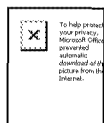
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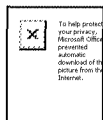
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To help protect
your privacy,
Microsoft Office
prevented
automatic
download of this
picture from the
Internet.

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Sent: Sunday, August 3, 2025 9:26 AM
To: Judith Curry
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Subject: Re: Hausfather Santer

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From: Park, Charles [charles.park@hq.doe.gov]
Sent: 8/4/2025 4:45:52 PM
To: curry.judith@gmail.com; Ross McKittrick [ross.mckittrick@gmail.com]; John Christy [climatemanager60@gmail.com]; Steven Koonin [steven.koonin@gmail.com]; Roy Spencer [roywspencer@hotmail.com]
CC: Ison, Jeremy T. [jeremy.ison@hq.doe.gov]
Subject: Contact Information

Good afternoon:

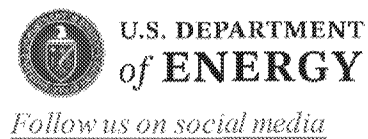
Thank you for your continued work on the report.

Moving forward, my colleague Jeremy Ison will be the point of contact.

Please direct all communications and inquiries to Jeremy Ison at jeremy.ison@hq.doe.gov.

Thank you,

Charles



Charles Park
Deputy General Counsel - Energy Policy
Office of General Counsel
OFFICE 202.586.5281
MOBILE 240.220.0969

From: Ross McKittrick <ross.mckitrick@gmail.com>
Sent: Wednesday, August 13, 2025 4:54 PM
To: John Christy
Cc: Roy Spencer; Judith Curry; Steven Koonin
Subject: Re: Fw: AP seeking your reaction to comments on DOE document from cited scientists and outside scientists
Attachments: image001.png

Ok I'll send and cc everyone. Busy with errands so it will be a bit later.

On Wed, Aug 13, 2025 at 4:50 PM John Christy <climateman60@gmail.com> wrote:
I also received the email and the response by Steve is fine by me. John C.

On Wed, Aug 13, 2025 at 3:19 PM Roy Spencer <roywspencer@hotmail.com> wrote:
That response looks good to me.
-Roy

Sent from my Verizon, Samsung Galaxy smartphone

----- Original message -----

From: Ross McKittrick <ross.mckitrick@gmail.com>
Date: 8/13/25 2:24 PM (GMT-06:00)
To: Judith Curry <curry.judith@gmail.com>, "Roy W. Spencer" <roywspencer@hotmail.com>, John Christy <climateman60@gmail.com>, Steven Koonin <steven.koonin@gmail.com>
Subject: Fwd: Fw: AP seeking your reaction to comments on DOE document from cited scientists and outside scientists

Did you all get this email from Borenstein?

AP emailed 350 scientists: 140 of whom weren't cited in the report. They got 59 responses, of which 50 were "negative" and 6 were "positive". They don't say what fraction of the 59 were cited in the report but 15 said their work was misused. 16 claimed there was cherry-picking.
The error on p. 90 we already know about.

Here are my suggested responses to their questions.

As we made clear in the report, it is not meant to be a comprehensive review of the whole of climate science, it is focused on data and topics we consider important and which we think have been under-reported or overlooked up to now.

We don't find generic accusations about bias or cherry-picking to be helpful. The report has been presented for public comment and expert review and we will consider all the feedback carefully as we make revisions.

Some errors, such as the one on page 90 about Arctic sea ice, have already been brought to our attention and will be corrected in the revised version.

Some degree of self-citation is inevitable since we are writing about topics within our areas of expertise.

From: Borenstein, Seth <SBorenstein@ap.org>

Sent: Wednesday, August 13, 2025 2:37 PM

To: Ross McKittrick <rmckitri@uoguelph.ca>

Cc: Phillis, Michael <MPhillis@ap.org>

Subject: AP seeking your reaction to comments on DOE document from cited scientists and outside scientists

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In 15 cases, cited scientists said the reports misused, misinterpreted or took their work out of context. Six scientists used the word “misleading” to describe the sections where their work was cited.

Among the many problems scientists pointed out, on page 90 the DOE report talked of a 5% decrease in Arctic sea ice since 1980, with a link to a document that clearly is marked Southern Hemisphere and it's for July only, not yearly average. On an annual basis Arctic – not Antarctic – sea ice has decreased more than 40% since 1980, NSIDC data shows. One researcher called the work sloppy, while another said the DOE section was misleading and actually false.

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“It is almost a user's guide on how to lie with figures,”

"The DOE and EPA reports are fundamentally flawed (mostly wrong and riddled with cherry-picked results that distort the scientific consensus). These documents do not reflect genuine scientific rigor, but rather a misleading reinterpretation of peer-reviewed research."

"The citation to our work was intentionally misleading,"

"I'm cited three times, once accurately."

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Does what mainstream scientists say about this matter?

And is there a reason why reporters authors referred more to their work than others, with more than two dozen McKittrick citations?

Thanks for your consideration.

Seth and Mike

Seth Borenstein

Science Writer

The Associated Press

1100 13th St. NW, Suite 500

Washington DC 20005 USA

<https://apnews.com/author/seth-borenstein>

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From: John Christy <climateman60@gmail.com>
Sent: Wednesday, August 13, 2025 4:50 PM
To: Roy Spencer
Cc: Ross McKittrick; Judith Curry; Steven Koonin
Subject: Re: Fw: AP seeking your reaction to comments on DOE document from cited scientists and outside scientists

I also received the email and the response by Steve is fine by me. John C.

On Wed, Aug 13, 2025 at 3:19 PM Roy Spencer <roywspencer@hotmail.com> wrote:
That response looks good to me.
-Roy

Sent from my Verizon, Samsung Galaxy smartphone

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Date: 8/13/25 2:24 PM (GMT-06:00)
To: Judith Curry <curry.judith@gmail.com>, "Roy W. Spencer" <roywspencer@hotmail.com>, John Christy <climateman60@gmail.com>, Steven Koonin <steven.koonin@gmail.com>
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DOJ_00047245

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I'm good with this response

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Judith Curry, President
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curry.judith@cfanclimate.com | +1.404.803.2012
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From: Steven Koonin [steven.koonin@gmail.com]
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To: 'Ross McKittrick' [ross.mckittrick@gmail.com]; 'Judith Curry' [curry.judith@gmail.com]; 'Roy W. Spencer' [roywspencer@hotmail.com]; 'John Christy' [climateman60@gmail.com]
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Yes, got that email as well. Here's a suggested edit of your response.

The report's preface states clearly that it is not meant to be a comprehensive review of climate science but rather is focused on important data and topics that have been under-reported or overlooked in media and political discussions. Generic accusations of bias or cherry-picking are not helpful for serious scientific discussions. The report has been presented for public comment and expert review and we will respond seriously to all serious criticisms, making revisions as warranted. Some errors, such as the one you cite about Arctic sea ice, have already been brought to our attention and will be corrected. Note also that some self-citation is inevitable since we are writing about topics within our areas of expertise.

And do make clear that you're responding on behalf of all the authors (if everybody else agrees).

SEK

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From: Borenstein, Seth <SBorenstein@ap.org>
Sent: Wednesday, August 13, 2025 2:37 PM
To: Ross McKitrick <rmckitri@uoguelph.ca>
Cc: Phillis, Michael <MPhillis@ap.org>
Subject: AP seeking your reaction to comments on DOE document from cited scientists and outside scientists

CAUTION: This email originated from outside of the University of Guelph. Do not click links or open attachments unless you recognize the sender and know the content is safe. If in doubt, forward suspicious emails to IThelp@uoguelph.ca.

Hello and we hope you are doing well.

Because of the discussion and controversy surrounding the July EPA proposed rescission of the Endangerment Finding and the Department of Energy's A Critical Review of Impacts of Greenhouse Gas Emissions on the U.S. Climate and claims by some scientists whose studies were cited that their work was misinterpreted by the documents, The Associated Press sent emails to more than 350 scientists this month, asking a series of questions. About 60% of those scientists had their work cited and the other 40% were outside scientists asked about the quality of the reports.

Here's a quick summary of our results. Can you please acknowledge receipt of this and respond by the end of business day Thursday as we plan to publish in the next few days.

Of the 59 responses we got, 50 were negative and six were positive. Scientists criticized the report for cherry-picking data and information – with 16 different scientists using a variation of the phrase cherry pick in 28 different instances. Scientists, both those from outside and those whose work was cited, said the reports were biased, misleading and do not reflect the current state of knowledge about climate change.

In 15 cases, cited scientists said the reports misused, misinterpreted or took their work out of context. Six scientists used the word “misleading” to describe the sections where their work was cited.

Among the many problems scientists pointed out, on page 90 the DOE report talked of a 5% decrease in Arctic sea ice since 1980, with a link to a document that clearly is marked Southern Hemisphere and it's for July only, not yearly average. On an annual basis Arctic – not Antarctic – sea ice has decreased more than 40% since 1980, NSIDC data shows. One researcher called the work sloppy, while another said the DOE section was misleading and actually false.

One scientist disputed the part in the DOE document that said fire acres burned increased “but only until about 2007” and said her calculations show a smaller increase post 2007. AP looked at NIFC acreage data and found three years since 2007 with more acres burned than 2007: 2015, 2017 and 2020. And the 10-year average of 2015-2024 is 7.5 million acres burned while the 10-year average from 1998-2007 is 6.5 million acres.

There are more. Here are a few quotes that we may be using in the story:

“It is almost a user's guide on how to lie with figures,”

“The DOE and EPA reports are fundamentally flawed (mostly wrong and riddled with cherry-picked results that distort the scientific consensus). These documents do not reflect genuine scientific rigor, but rather a misleading reinterpretation of peer-reviewed research.”

“The citation to our work was intentionally misleading,”

“I'm cited three times, once accurately.”

Of course there were six scientists who praised the work and we will reflect that.

We seek your thoughts on what we found and what the scientists are saying. Do you consider your report biased? If not, why was it written by only known climate change skeptics?

Does what mainstream scientists say about this matter?

And is there a reason why reporters authors referred more to their work than others, with more than two dozen McKittrick citations?

Thanks for your consideration.

Seth and Mike

Seth Borenstein

Science Writer

The Associated Press

1100 13th St. NW, Suite 500

Washington DC 20005 USA

<https://apnews.com/author/seth-borenstein>

@borenbears on X

@borenbears.bsky.social

Emails preferred: sborenstein@ap.org

+1-202-641-9454

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"There are only two forces that can carry light to all corners of the globe -- only two -- the sun in the heavens and The Associated Press down here."

- Mark Twain

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From: Woods, Andrea <andrea.woods@hq.doe.gov>
Sent: Monday, August 4, 2025 4:52 PM
To: Steven Koonin; Barrios, Audrey; Loucks, Joshua; Cohen, Seth; Park, Charles
Cc: curry.judith@gmail.com; 'John Christy'; roywspencer@hotmail.com; 'Ross McKittrick'
Subject: Re: [EXTERNAL] FW: A Critical Review of Impacts of Greenhouse Gas Emissions on the U.S. Climate

Thanks Steve. Adding Charles Park from GC. [REDACTED]

From: Steven Koonin <steven.koonin@gmail.com>
Date: Monday, August 4, 2025 at 12:34 PM
To: Woods, Andrea <andrea.woods@hq.doe.gov>, Barrios, Audrey <audrey.barrios@hq.doe.gov>, Loucks, Joshua <Joshua.Loucks@hq.doe.gov>, Cohen, Seth <seth.cohen@hq.doe.gov>
Cc: curry.judith@gmail.com <curry.judith@gmail.com>, 'John Christy' <climateman60@gmail.com>, roywspencer@hotmail.com <roywspencer@hotmail.com>, 'Ross McKittrick' <ross.mckittrick@gmail.com>
Subject: [EXTERNAL] FW: A Critical Review of Impacts of Greenhouse Gas Emissions on the U.S. Climate

DOE folks:

As I expect DOE would be happy to see translations, I'm forwarding this in the hopes you could accelerate approval.

I suspect there will be other requests for translations into other languages.

SEK

From: Franco Battaglia <francesco.prof.battaglia@gmail.com>
Sent: Monday, August 4, 2025 5:13 AM
To: The.Secretary@hq.doe.gov; Steven E Koonin <steven.koonin@nyu.edu>
Subject: A Critical Review of Impacts of Greenhouse Gas Emissions on the U.S. Climate

To Whom it may concern.

I'm a university professor of chemical physics (PhD from the university of Rochester, NY) and co-founder of the Clintel Foundation (<https://clintel.org/>). I'm also the author of the book "There is no climate emergency", <https://www.amazon.com/dp/8887731780>).

I'm writing to ask you for permission to translate into Italian the Report "A Critical Review of Impacts of Greenhouse Gas Emissions on the U.S. Climate". I'll make the translation together with my Italian colleague prof. Nicola Scafetta, whose work has been cited several times in the Report. Please ask prof. Steve Koonin for references (I'm sharing this email with him).

I should add that the translation will be a no-profit engagement (I will do it on a voluntary basis and will not be paid for that).

Thank you for your attention,

Franco Battaglia

This message does not originate from a known Department of Energy email system.
Use caution if this message contains attachments, links or requests for information.

From: Ross McKittrick <ross.mckitrick@gmail.com>
Sent: Monday, August 4, 2025 1:30 PM
To: Steven Koonin
Cc: Woods, Andrea; Barrios, Audrey; Joshua Loucks; Seth Cohen; curry.judith@gmail.com; John Christy; roywspencer@hotmail.com
Subject: Re: FW: A Critical Review of Impacts of Greenhouse Gas Emissions on the U.S. Climate

I approve

On Mon, Aug 4, 2025 at 12:34 PM Steven Koonin <steven.koonin@gmail.com> wrote:

DOE folks:

As I expect DOE would be happy to see translations, I'm forwarding this in the hopes you could accelerate approval.

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Thank you for your attention,

Franco Battaglia

From: Steven Koonin <steven.koonin@gmail.com>
Sent: Monday, August 4, 2025 12:35 PM
To: 'Woods, Andrea'; 'Barrios, Audrey'; 'Joshua Loucks'; 'Seth Cohen'
Cc: curry.judith@gmail.com; 'John Christy'; roywspencer@hotmail.com; 'Ross McKittrick'
Subject: FW: A Critical Review of Impacts of Greenhouse Gas Emissions on the U.S. Climate

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I should add that the translation will be a no-profit engagement (I will do it on a voluntary basis and will not be paid for that).

Thank you for your attention,

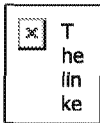
Franco Battaglia

From: Judith Curry [curry.judith@gmail.com]
Sent: 8/5/2025 8:48:44 PM
To: Travis Fisher [travis.scott.fisher@gmail.com]; Josh Loucks [loucksj14@gmail.com]; Cohen, Seth [seth.cohen@hq.doe.gov]; Barrios, Audrey [audrey.barrios@hq.doe.gov]
CC: Ross McKittrick [ross.mckittrick@gmail.com]; Steven Koonin [steven.koonin@gmail.com]; Roy Spencer [roywspencer@hotmail.com]; John Christy [climateman60@gmail.com]
Subject: Response to the internal DOE reviews
Attachments: DOE review responses final.zip

The final responses to the internal DOE reviews are attached in a zip file

.....|

--



Judith Curry, President
CFAN - Climate Forecast Applications Network
Reno, NV USA
curry.judith@cfanclimate.com | +1.404.803.2012
<http://www.cfanclimate.net>

Pg	Para	Comment or Suggested Edit	
viii	2	The greening effect can't be due to increase in CO2 alone. The fertilization effect needs Nitrogen and phosphorous. There is observational[1] evidence that nitrogen available to plants is decreasing with increasing CO2 and thus acting to limit fertilization effect. Secondly, the greening of some portions of the planet are likely due to changes in precipitation patterns associated with changing climatic conditions and not entirely attributable to fertilization effect. We will also need to consider that the temperature effect on photosynthesis will also be a limiting mechanism to any continued greening due to fertilization in the future [2]. I recommend acknowledging that the greening is not attributable to CO2 alone and continued greening in the future with increasing CO2 is uncertain. Changed sentence to “Elevated concentrations of CO₂ directly enhance plant growth, globally contributing to “greening” the planet”	[1] Rachel E. Mason et al. Evidence, causes, and consequences of declining nitrogen availability in terrestrial ecosystems. Science 376, eabh3767(2022).DOI:10.1126/science.abh3767 [2] C.E. Moore, et al. The effect of increasing temperature on crop photosynthesis: from enzymes to ecosystems. Exp. Bot., 72 (8) (2021), pp. 2822-2844
viii	3	I am glad we acknowledge that CO2 causes warming, though I am not sure why we are not considering other energy sector related emissions that also cause warming (CFCs/HFCs, methane, aerosol effects). It is not clear what is meant by "CO2 emissions have tended to overestimate observed trends". This is not correct, the cumulative emissions track the RCP8.5 scenario the closest [3,4]. While there was a decrease in emissions during the Covid-19 period the emissions are now back on track following RCP8.5. This statement should clarify what is meant by the statement in the text. Rewrite sentence in the summary section to read “Climate change simulations require scenarios of distant future emissions. There is evidence that scenarios widely-used in the impacts literature have overstated observed and likely future emission trends.” On page 16 text added “Schwalm et al. (2020) defended the use of RCP8.5 on the grounds that cumulative CO2 emissions over 2005-2020 track it more closely than the lower RCP scenarios. They also argue that a modified version of the IEA scenarios closely track RCP8.5 in the coming decades. Hausfather and Peters (2020b) responded that the skill of RCP8.5 over those 15 years is due to offsetting errors in its representation of CO2 from fuel use and land use change, and the apparent agreement with IEA in coming decades is due to Schwalm et al. adding in very high land use emissions. The IEA’s own projected CO2 emissions track well below RCP8.5.”	[3] C.R. Schwalm, S. Glendon, & P.B. Duffy, RCP8.5 tracks cumulative CO2 emissions, Proc. Natl. Acad. Sci. U.S.A. 117 (33) 19656-19657, https://doi.org/10.1073/pnas.2007117117 (2020). [4] C.R. Schwalm, S. Glendon, & P.B. Duffy, Reply to Hausfather and Peters: RCP8.5 is neither problematic nor misleading, Proc. Natl. Acad. Sci. U.S.A. 117 (45) 27793-27794, https://doi.org/10.1073/pnas.2018008117 (2020).

viii	4	This paragraph seems to be talking about equilibrium climate sensitivity. The most recent model inter-comparison gives a wider range of climate sensitivities in the range of 1.8 to 5.7 and is wider than estimates of climate sensitivities that have been estimated from paleoclimatic and more recent historical datasets, in the range could be in the range of 2.3 to 4.5 K[5]. The higher sensitivity in the most recent model assessments are an area of active investigation by modelers, the feasibility of a higher range needs to be confirmed with additional observational and process scale modeling [6]. The range for climate sensitivities, 1.8-2.7, should be corrected to the most recent findings, 2.3-4.5. We should also recognize that the (a)earth is a complex system of systems (b)we can't do an experiment to find out what is correct or reduced the uncertainty. As we get better at extracting temperature records and CO2 concentrations from paelo data, there will be better reconstructions and possibly narrower range along with improvements in modeling that resolve many physical, biological and chemical processes as we get to higher and cloud resolving spatial resolutions. We discuss Sherwood et al. in Ch 4. Sentence changed: “Data-driven methods yield a lower and narrower range.”	[5] Sherwood, S. C., Webb, M. J., Annan, J. D., Armour, K. C., Forster, P. M., Hargreaves, J. C., et al. (2020). <u>An assessment of Earth's climate sensitivity using multiple lines of evidence</u>. <i>Reviews of Geophysics</i>, 58, e2019RG000678. https://doi.org/10.1029/2019RG000678 [6] Zelinka, M. D., Myers, T. A., McCoy, D. T., Po-Chedley, S., Caldwell, P. M., Ceppi, P., et al. (2020). <u>Causes of higher climate sensitivity in CMIP6 models</u>. <i>Geophysical Research Letters</i>, 47, e2019GL085782. https://doi.org/10.1029/2019GL085782
viii	5	This is again not true for the majority of the models in the AR6. Figure 7.19 in the IPCC AR6 report shows that majority of the models exhibit a range within the observational range. The models running hot in the most recent decades are the ones with climate sensitivities grate than 5K, which are 3 out of a total of 40 and it should be noted that about the same number of models also are running cold' compared to observed[7] Paragraph has been rewritten: The world’s several dozen global climate models offer little guidance on how much the climate responds to elevated CO , with the average surface warming under a doubling of the CO concentration ranging from 1.8°C to 5.7°C [Section 4.2]. Data-driven methods yield a lower and narrower range [Section 4.3]. Global climate models generally run “hot” in their description of the climate of the past few decades–too much warming at the surface and too much amplification of warming in the lower- and mid- troposphere [Sections 5.2-5.4]. The combination of overly sensitive models and implausible extreme scenarios for future emissions yields exaggerated projections of future warming.	[7]Hausfather, Z., Drake, H. F., Abbott, T., & Schmidt, G. A. (2020). Evaluating the performance of past climate model projections. <i>Geophysical Research Letters</i>, 47, e2019GL085378. https://doi.org/10.1029/2019GL085378
viii	5,6,7	There is very little dispute that these are challenging issues for climate science. Many of the issues identified here related to compound effects (subsidence and se-level rise; wildfire and forest management) that requires further scientific investigation. Identifying further research avenues/focus areas will be helpful to firm up some of the challenges faced in using the climate projections as discussed in the paragraph 9. There is no disagreement here.	

viii	9	The focus on uncertainty is relevant and should be emphasized in future research. However, as a reasonable policy pathways, making decisions in the face of these uncertainties is critical. Most of the climate uncertainties that we have confidence at this time can be considered Level 2 and Level 3 uncertainties, approaches such as scenario analysis (and others) are suggested and followed for making hazard and risk estimates for future conditions. For deep uncertainty hazards (for example tornadoes etc.) that we don't have enough data or capability to model, developing methods for assessing the uncertainty and risk should be a focus of future research. There is no disagreement here. See Curry's book Climate Uncertainty and Risk.	
2	3	The reason CO2 was identified as a pollutant is the health consequences of increased temperature, flooding and other climate impacts and not because of any direct affect on health dur to exposure to atmospheric CO2[8]. There is no disagreement here.	[8] https://www.cdc.gov/climate-health/php/effects/index.html
3	1/C hap ter su m ma ry	The summary should note that CO2 fertilization effects are limited by the availability of other critical plant nutrients (Nitrogen, Phosphorous). The fertilization effect is also constrained by higher temperature effecting photosynthesis[9] Added sentence: Other contributing factors included warming and increased nitrogen.	[9] Xinyuan Wei et al 2025 Environ. Res. Lett. 20 064013
3	3	The attribution of greening to primarily CO2 fertilization is questionable as per recent research. Specifically, in the Northern high latitudes (where the greening is most prominent), increases in temperatures due to climate change has been shown to provide the best explanation[10]. In the sub-Saharan region reforestation efforts and changing precipitation patterns lead to some of the observed greening (the great green wall) [11]. The attribution to CO2 fertilization should be qualified by noting the other present and emerging constraints. We cite Zhu et al attributing 70% of change to CO2 . That implies other factors matter as well.	[10]Liu, J., Wennberg, P.O. An emergent constraint on the thermal sensitivity of photosynthesis and greenness in the high latitude northern forests. Sci Rep 14, 6189 (2024). https://doi.org/10.1038/s41598-024-56362-1 [11] Francesco S.R. Pausata, Marco Gaetani, Gabriele Missouri, Alexis Berg, Danielle Maia de Souza, Rowan F. Sage, Peter B. deMenocal, The Greening of the Sahara: Past Changes and Future Implications, One Earth, Volume 2, Issue 3, 2020, Pages 235-250, ISSN 2590-3322, https://doi.org/10.1016/j.oneear.2020.03.002 .
3	5	This summarization ignores the compound effects of temperature, moisture and CO2 fertilization in assessing the impacts of CO2 increase on vegetation and crop yields. Many studies have shown that [12]. Making note of these compensating factors is required for a balanced view on CO2 fertilization effects on plants. This issue is addressed in Chapter 9	[12]Lesk, C., Anderson, W., Rigden, A. et al. Compound heat and moisture extreme impacts on global crop yields under climate change. Nat Rev Earth Environ 3, 872–889 (2022). https://doi.org/10.1038/s43017-022-00368-8

4	1	Chen et al. (2024)[13] conclude that "In the attribution analysis of LAI trend and growth rate trend, there were some differences between multiple linear regression model and partial correlation analysis at high latitudes in the northern hemisphere. Partial correlation analysis showed that LAI trend and growth rate trend in this region were mainly affected by temperature and radiation, while multiple linear regression suggested that CO2 was the dominant driver. The results of the partial correlation analysis seem more reasonable because some previous studies have shown that vegetation change in the high latitudes of the northern Hemisphere is mainly positively affected by climate warming. Multiple linear regression model performed relatively poorly in this region and might not fully identify the contribution of individual drivers. In addition, our model does not include the contribution of land management, which has been highlighted in some previous studies. Of course, the contribution of land management to LAI trend and growth rate trend may be implied in other drivers, such as the positive contribution of CO2 to LAI trend in PP and PN areas". We have added the Chen references. Our existing summary doesn't conflict with this. They conclude CO2 is the dominant driver globally but not the only driver.	[13]Xin Chen, Tiexi Chen, Bin He, Shuci Liu, Shengjie Zhou, Tingting Shi, The global greening continues despite increased drought stress since 2000, <i>Global Ecology and Conservation</i>, Volume 49, 2024, e02791,ISSN 2351-9894,
6	1	This again suffers from not acknowledging the effects of compound and often compensating phenomena that is occurring with the on-going climate change. For example [14] note that much of the Midwest any benefit from CO2 fertilization will be negated by temperature increases. [15] note that "Our results show that long-term physiological acclimation dampens the initial stimulation of plant net carbon assimilation to elevated [CO2], and of plant water use to warming." The topic of agricultural productivity is addressed in Chapter 9 It is not disputed that effects of dT go negative at high enough levels. The 2013 paper by Ruiz-Vera et al doesn't provide much general information beyond the FACE experiment site. The C14 data set used is a global sample and takes both dT and dC into account.	[14]Ursula M. Ruiz-Vera, Matthew Siebers, Sharon B. Gray, David W. Drag, David M. Rosenthal, Bruce A. Kimball, Donald R. Ort, Carl J. Bernacchi, Global Warming Can Negate the Expected CO2 Stimulation in Photosynthesis and Productivity for Soybean Grown in the Midwestern United States , <i>Plant Physiology</i>, Volume 162, Issue 1, May 2013, Pages 410–423, https://doi.org/10.1104/pp.112.211938 [15] Lamba S, Hall M, Rantfors M, et al. Physiological acclimation dampens initial effects of elevated temperature and atmospheric CO2 concentration in mature boreal Norway spruce. <i>Plant Cell Environ.</i> 2018; 41: 300–313. https://doi.org/10.1111/pce.13079
8	1	The fallacy of these arguments in terms of PH changes and evolution is that the changes described in this paragraph occurred over millennia and the evolutionary process had time to adapt; here with climate change we are talking about changes in our lifetime, the speed of change is the issue. This will lead to collapse of some ecosystem and by the time adaptation takes place it will be millions of years and in a different climate - not the one we have now and that enabled the rise of humans. The chapter notes large regional and even diurnal variations in ocean pH. We already address the issue of short timescale variations on p. 8: "On the time scale of thousands of years, boron isotope proxy measurements show that ocean pH was around 7.4 or 7.5 during the last glaciation (up to about 20,000 years ago) increasing to present-day values as the world warmed during deglaciation (Rae <i>et al.</i>, 2018). Thus, ocean biota appear to be resilient to natural long-term changes in ocean pH since marine organisms were exposed to wide ranges in pH."	

5	Fig 2.2	A simple calculation will show us how misleading this figure is. It shows that over 600 Million years CO2 changed by around 6000ppm. That is a rate of change of 0.00001/yr; COE changed from 275 to 430 in the past 175 years, that is rate of change of 0.88ppm/yr. A change in the rate of over 10^5 in magnitude - there in lies the problem. We are seeing a rapid increase in CO2 that has never been witnessed and the ecosystem and huma society (which is at best 10K years old), may not be able to adapt to this rapid change. This figure has been deleted. We don't know what the rate of changes was on an annual or centennial time scale since the data are too low resolution. We have added the following sentence “While the modern rate of change in CO2 is likely high compared to prior intervals, the geological evidence is that plants and animals evolved under much higher CO2 levels than at present.”	
12	Summary	The misuse of RCP8.5 as business as usual is acknowledged. However, this was never intended to be a no-policy or business as usual scenario but as a high-end baseline scenario. When dealing with interaction of physical systems with socio-economic projections (which tend to be highly uncertain and policy dependent), the development of this high baseline scenarios is intended to explore the uncertainty ranges that represent a high-end scenario[16,17]. As any engineer would acknowledge, this is a prudent approach when dealing with L3 and L4 type deep uncertainties and provide valid envelopes for assessing uncertainties and risks from high impact events/hazards. The rest of the statements here are uncertainties acknowledged by the climate scientists and included in developing projections and discussing the results[4]. Does the IPCC downplay the role of sun in climate change? The IPCC includes sun variability in all its assessments from the beginning and has been acknowledged as a potential but insignificant contributor. The comments do not imply any disagreement with what we have written. Extreme scenarios for models are useful for bracketing outcomes but our criticism (based on the literature) is the widespread use of RCP as a no-policy baseline; the reviewer acknowledges that was never its purpose. The IPCC's assessment insignificance of the sun in climate change is based in part on use of low variability irradiance reconstructions that rule out it having any role. This is addressed in Chapter 8	[16]van Vuuren, D.P., Edmonds, J., Kainuma, M. et al. The representative concentration pathways: an overview. Climatic Change 109, 5 (2011). https://doi.org/10.1007/s10584-011-0148-z [17]Hausfather, Z. (2025). An assessment of current policy scenarios over the 21st century and the reduced plausibility of high-emissions pathways. Dialogues on Climate Change, 2(1), 26-32. https://doi.org/10.1177/29768659241304854 (Original work published 2025)
16	2,3, 4,5	Please see above. The business-as-usual may be a misnomer but this is an 95% case that is typically included in any type of prudent uncertainty analysis, in particular given the difficulty of predicting the evolution of socio-economic and political conditions over the next 100 years. It should be noted the difference between RCP8.5 and RCP4.5 for mid-century conditions that is of particular focus for adaptation studies in not significant and most of the divergence in these two scenarios occurs later in the century. Implausibility is not equivalent to very unlikely (e.g. 95th percentile), but rather includes impossible or implausible assumptions. High impact hazards occur independent of atmospheric CO2 concentrations (Chapter 6).	

19	5	That the dynamic vegetation models need more development is acknowledged and is a defining challenge for Earth System Modeling for the next decade. There is no disagreement here	
22	3	As noted, Parker[18,19,20] has investigated the Urban effects on temperature records. The finding establish that by comparing warm and wind conditions world wide that the impact is negligible. However, as noted by authors, this literature hasn't progressed much over the last decade and much more attention needs to be paid to this question by employing newer models, datasets and analytical techniques available to us at present. The CWG Report addresses this concern. We agree with the reviewer that the literature stalled a decade+ ago.	[18] Parker, D. Large-scale warming is not urban. <i>Nature</i> 432, 290 (2004). https://doi.org/10.1038/432290a [19]Parker, D.E. (2010), Urban heat island effects on estimates of observed climate change. <i>WIREs Clim Change</i>, 1: 123-133. https://doi.org/10.1002/wcc.21. [20]Parker, D. E., 2006: A Demonstration That Large-Scale Warming Is Not Urban. <i>J. Climate</i>, 19, 2882–2895, https://doi.org/10.1175/JCLI3730.1.
28	2	We can all agree that there is a desperate need for developing higher-resolution climate models that can get to cloud resolving scale so that we can represent the cloud feedbacks correctly in the model and avoid uncertainties from parameterization and scale dependency of the parameters. This doesn't make these models useless. The models provide directions for further improvements and necessary changes to reduce the uncertainty bands over the ECS. The climate models are useful for research purposes. The CWG Report demonstrates that they are not fit for the purpose of policy making. There's no guarantee increased resolution will be the answer, but if ever a model appears that can deal correctly with clouds that will be quite something. .	
29	2	The databased approaches have mostly been robust with changes at the high end around 3-4C and at the low end around 1.8 to 2.5C as noted in the chapter. It will be challenging to make this range shorter given the data requirements and uncertainties in data over paleo/haloocene periods. It seems reasonable to estimate that we may have ECS in the range of 3 to 4K (higher than 3.5K being less likely [21]) at the higher end and this should not prevent us from planning and thinking on ways to prevent this possible scenario as it could have major consequences for humans across the planet. The Report acknowledges that 4C is a reasonable upper bound for the likely range	[21] Michael Mann, <i>Our Fragile Moment: How lessons from Earth's past can help us survive climate crisis</i>, Hachette Group Inc., 2024.

32	3	The models range over a factor of three in ECS comparing the most sensitive to the least sensitive models. However vast majority of the models are in 3 to 3.5K sensitivity range and it is probably not correct to assign all models the same amount of importance, given the fact that some of institutes participating in the exercise have different levels of capacity to develop and test models. Only 5 of the models are between 3 and 3.5C Some of the most reputable models have the highest values of ECS, including UK and US models The point made in this paragraph is that research over several decades has not reduced the range of model ECS. For observers who rely on model outputs this is very concerning and again goes to the question of fitness for purpose, as posed in the prior paragraph.	
34	1	While this analysis for the CMIP6 model set is accurate and focuses on a dataset that has shown higher ECS than previous iterations. it should be noted the models have exhibited considerable skill in predicting the subsequent GMST warming[22]. The criticism of the CMIP6 based high ECS models may be premature and probably will have to wait additional time before passing judgement. As noted in [22] these models have shown considerable skill when evaluated on their projections over the past 30-40 years after the projections were published. <i>Figure 5.3 in the Report clearly refutes this assertion</i> Hausfather et al. did examine early model-obs matches but only using models that had relatively low ECS. The Scaffeta analysis covered a longer interval (1980-2021) than the Hausfather et al. analysis of SAR (1995-2017) and TAR (2001-2017). Its findings line up with Fig 5.3 and the tropospheric evidence in Fig 5.4.	[22]Hausfather, Z., Drake, H. F., Abbott, T., & Schmidt, G. A. (2020). Evaluating the performance of past climate model projections. <i>Geophysical Research Letters</i>, 47, e2019GL085378. https://doi.org/10.1029/2019GL085378
36	1	A recent study attributes the bias to inter-annual availability and residual bias in the satellite record. The pre- and post 2000 have temperature records from two separate satellites and the discrepancy in the trends needs to be resolved[23] Chung et al. (using output through 2014 only) confirm the models' too-high warming rates and suggested two issues that might contribute to at least part of the discrepancy: (1) the differential phasing of natural, ephemeral events such as El Niños between models and observations and (2) a possible spurious cooling in UAH and NOAA/STAR satellite datasets around 2000. Regarding (1) we agree that the timing of major events like ENSOs diminishes the fidelity of models-to-observations comparisons for short periods, but now with 46 years of record as well as two major El Niño warm events since 2014 in the observations, the results shown in Fig. 5.7 are quite appropriate for comparison. Regarding (2) Christy et al. 2018 (and later Zou et al 2023) specifically examined the divergence around 2000 between UAH and NOAA/STAR vs. RSS and UW demonstrating that RSS and UW's use of the aging and drifting NOAA-14 satellite relative to the new, highly calibrated NOAA-15 sensor contributed to a spurious warming in RSS and UW. Be that as it may, CWG Fig. 5.7 is clear in noting all models overestimate the tropical temperature trend relative to observations – model trends being even more positive than those of RSS and the two Reanalyses (in addition to UAH and NOAA/STAR).	[23]Chung, ES., Kim, SJ., Sohn, BJ. et al. Multi-decadal climate variability and satellite biases have amplified model-observation discrepancies in tropical troposphere warming estimates. <i>Commun Earth Environ</i> 5, 342 (2024). https://doi.org/10.1038/s43247-024-01510-8

38	3	This is challenge we need to face and work on improving the parametrizations. The new era of super high resolution models would resolve the complex interactions of turbulence, transport and clouds and will provide less uncertainty[24]. However, this can't be used to invalidate the entire set of models, the models are performing remarkably well given the complex system that is being modeled. It should be noted the models do cover the range of the observations and it is not outside the envelope of model predictions. The Report does not address invalidating the entire set of models. Rather we address the fitness-for-purpose of the models for policy making Fig 5.6 shows that to the extent there is a match with observations, it comes from models with very little CO2 response. “However, this can't be used to invalidate the entire set of models, the models are performing remarkably well given the complex system that is being modeled.” This statement is not supported by the evidence unless and until “remarkably well” is metricized for us to examine and make a determination. The paper cited doesn’t exactly apply to the real world (looking at Fig 1 doesn’t give one much confidence in models), but does give insight into the various models as best I can tell.	[24]Merlis, T. M., Guendelman, I., Cheng, K.-Y., Harris, L., Chen, Y.-T., Bretherton, C. S., et al. (2024). The vertical structure of tropical temperature change in global storm-resolving model simulations of climate change. Geophysical Research Letters, 51, e2024GL111549. https://doi.org/10.1029/2024GL111549
40	4	Part of the problem here may be which part of the stratosphere is sampled and the importance of interannual a variabilities in the lower part of the stratosphere. [25] have shown that including the stratosphere as defined from 25 to 50km there is high confidence that the cooling signal caused by CO2 increase is detectable and beyond the noise levels of the natural and inter annual variability in the lowest part of the stratosphere. We should put this debate to rest. Santer et al. (2023 has been added to the references with this text: Santer et al. (2023) use updated data to show that a cooling trend has not re-emerged in the lower stratosphere.	[25] B.D. Santer, S. Po-Chedley, L. Zhao, C. Zou, Q. Fu, S. Solomon, D.W.J. Thompson, C. Mears, & K.E. Taylor, Exceptional stratospheric contribution to human fingerprints on atmospheric temperature, Proc. Natl. Acad. Sci. U.S.A. 120 (20) e2300758120, https://doi.org/10.1073/pnas.2300758120 (2023).
42	2	Given the large uncertainties in obtaining data over much of the Northern Hemisphere, using a smaller regional analysis to discount the models may be inappropriate. We should that this remains a question we need to resolve developing better datasets with more seasonal and sub-seasonal coverage to test the model on a global scale The reviewer’s points are fine but it is important in the context of this report to point out discrepancies in the way models on some key continental-scale metrics such as, in this case, snow cover. This section directly addresses a region that is important to estimates of the Social Cost of Carbon in the context of agricultural productivity, again reiterating that the models are not fit for the purpose of policy making.	

44	3	This statement from [26] refers to the fitness of global scale models to resolve regional scale features and argues for developing model at higher spatial resolutions. Using downscaling and other techniques additional insights can be gained at regional scales and provide information for a set of 'purposes'. However, we should work on getting to regional and local scales with GCM as quickly as possible. The current model structural forms may not be adequate for resolving all of these issues merely by increasing spatial resolution. Simulating change at regional to local scales depends on getting the internal variability of the ocean correct, which is only partly a resolution issue.	
47	2	Looking for trends or estimating trends in extreme events in current and future climate is not necessarily an attribution study. Estimating frequency, duration and intensity of events helps understand the changing range of the hazard (if there is one). An attribution would be to find to what extent a particular event is influenced or affected by increase in CO2. I would consider these two not to be the same. We explain the distinction between detection and attribution	
47	3	Attributing any single event to changing climate is perilous, agreed. Yes. .	
47	5	As noted by [26] there is not yet any strong evidence that LPT is a fact We have eliminated mention of LPT explicitly, but retain elements of the text surrounding long term variability.	[26]Cohn, T. A., and H. F. Lins (2005), Nature's style: Naturally trendy, Geophysics. Res. Lett., 32, L23402, doi:10.1029/2005GL024476.
57	2	I am not sure how one can conclude that the temperature extremes have decreased based on figure 6.3.4. If I understand correctly the Tmax-Tmin for each year was tabulated and a mean of this difference averaged over the 100+ years was subtracted from these values to get some thing like a anomaly. This anomaly seems to have moved from +3F to -3F over the course of q00 years and now trending upwards again. This suggests to me that there has been significant changes in the extremes over the years and the most recent trend is towards warmer highs? Figure 6.3.3 clearly shows cold extremes have decreased on a centennial scale while warm extremes haven't changed much In Figure 6.3.4, for each station and each year, the hottest day and coldest night are determined and their difference computed. These differences are then geographically averaged for the CONUS and then an overall average is removed to show the results as a type of anomaly. Reviewer 1 is correct, as indicated in the text, Tmin temperatures are almost universally rising more rapidly than Tmax, hence, the difference (or range) has declined overall. A check of CMIP6 models for the CONUS under ssp4.5 shows half with TMax rising faster than Tmin and (obviously) half vice versa – so there is no clear signal from models that matches the observations. The flattening (or slight rise in the metric) in the past 20 years is clearly within the natural range, but does reveal an interesting stair-step pattern with rapid declines centered around the 1940's and 1990's and relatively flat periods in between. An explanation is under	

		investigation, and a leading hypothesis is that there was rapid growth of manufactured surfaces around weather stations in the 1940s. Then, in the 1960s-80s many stations were re-located to relatively rural airports, which then experienced subsequent growth in manufactured surfaces around their weather stations in the 1990s. Thus, rapid nighttime warming in the 1940s and 1990s.	
58	3	The NCA5 is suggesting that the western US is experiencing increase in extreme heat (page 57, para 4); it seems like figure 6.3.5 establishes the fact and not contradict. There was no claim of pervasive (all CONUS) increase in the report We have deleted the sentence “The NCA5 heading “The Risk of Temperature Extremes is Changing” suggests pervasive positive trends are now being observed in threshold temperatures, but that is not evident in the figures above.” It isn’t worth going into more detail in this report	
59	4	The definition of heat wave as consecutive 6 days seems to be a special case. The standard definition is exceedance of 85 or 90 percentile for 2 or more days. I can't check with NCA5 as it was taken down from the web As correctly suggested by Reviewer 1, there are numerous ways to define a heat wave. CWG chose to follow one definition discussed in NCA4 (pg 191) and use 6 consecutive days with Tmax > 90th percentile so as to allow for consistent comparisons. However, changing the parameters will do little to alter the fundamental result that the 1920's to early 1950's contain the major heat wave events for the CONUS.	
60	1,2	Figure 6.3.6 establishes that heatwaves as defined here have increased in the west and have shown increasing trend all over CONUS in the past 50 years. [27] shows the results for the last 60 years and there is little doubt that there is an increase over most of the CONUS, again the NCA5 report is off the web; figure 6.3.7 shows the heat waves are monotonically increasing from the 1960. Including the longer data is only interesting in the dust bowl 1930 camouflage the signal somewhat for the Midwest and central plains. The problem of taking averages over large regions (for example entire CONUS) is that it kills the peak signals and smooths out the emergent climate change indicators. Doing continental averages to look for extremes is really not very useful. Secondly, there is no particular reason to assume that the climate change signal (i.e. the number of heat waves) should showing up 100 years ago, as the CO2 in the atmosphere started to be of significance in the last 50 years. As discussed in the CWG text, the EPA chart is (1) based on nighttime temperatures in the 50 largest US cities and (2) begins in 1961 which was the start of the coolest decade in over 100 years, thereby preconditioning the chart to have an upward trend. Urbanization is most clearly seen in nighttime temperature increases so this does not represent a change in the background climate, but more likely the local climates of cities. Rural areas show less nighttime warming. And, avoiding earlier warm periods does not provide information on the larger scale of natural variability. We have added a sentence at the end of Section 6.3 “Irrespective of the ultimate cause of regional trends, heatwaves have important effects on society which must be addressed, as we discuss in Chapter 10. “	[27] https://www.epa.gov/climate-indicators/climate-change-indicators-heat-waves

61	4	averaged over all stations in the region, none of these trend parameters is statistically significant'. This is again how one would be looking at statistics of extremes in precipitation. As we know, precipitation is highly heterogenous and location dependent. As observed there are trends even with the approaches chosen by the authors of this study compared to NCA5 and averaging that over and saying there is no trend seems disingenuous. We summarize the site-specific trends as well as the regional average. The rationale for looking at the regional average is that a GHG signal should cover a wide area.	
62	2	same argument as above We summarize the site-specific trends as well as the regional average. The rationale for looking at the regional average is that a GHG signal should cover a wide area.	
63	box	again the issue here is that we are talking about events that are happening in a short-time and over a small region. Averaging over time and space and develop statistics is probably not appropriate when we are talking about events such as the recent flooding event in Texas. If you average that event over 30 days, it will seem unremarkable. Extreme event totals are 1-day to 5-day,s depending on section discussed. The point being made in the Box is that if you use a too-short record you may underestimate natural variability. I'm not sure how the reviewer's comment challenges this.	
64	1	As noted in NCA5 and shown in the analysis presented in figure 6.4.4 there is a clear trend of increase in extreme precipitation in 12 of the 27 stations analyzed. Again averaging over the entire region is not the best method for picking extreme precipitation events that are local both in time and space. We summarize the site-specific trends as well as the regional average. The rationale for looking at the regional average is that a GHG signal should cover a wide area. Also in this case we point out that the NE behaves differently than other regions.	

69	2	However, there is evidence that flash droughts, which last weeks have increased over the Northeast and Midwest [28] The paper referenced by Reviewer 1 begins with data so recent (1999) that it cannot provide evidence for long-term changes in drought as evidenced in the US Drought Monitor (DM) even though it suggests an increase in "flash droughts" over that period. Keep in mind, the DM is a product of subjective interpretation and objective measures - objective measures that have changed considerably during the past 25 years – thus is not metrically consistent. (One of the CWG authors was an official contributor to the DM for 15 years.) One would wonder what would the 1920's. 30's and early 50's reveal if the DM was operational back then? A more consistent metric is provided in CWG Fig. 6.7.1.	[28]Ronald D. Leeper, Rocky Bilotta, Bryan Petersen, Crystal J. Stiles, Richard Heim, Brian Fuchs, Olivier P. Prat, Michael Palecki, Steve Ansari
70	2	More recent analysis indicate forest loss due to wildfire accelerating over the recent past[29]. In particular there has been an increase in extreme fire radiative power with a 2 fold increase from 2003 to 2023 (period of satellite data availability)[30][31]. However, that these fires are not out-of-sample and there is a fire deficit as shown in figure 6.8.2 should be acknowledged. But this doesn't alleviate the concern that wildfire is leading to lot of socio-economic concerns and needs to be addressed[32] New sentence added: However there is evidence that the intensity of fires in some regions is worsening (Cunningham et al. 2024) and that wildfires resulted in a net loss of global forest cover over 2001-2019 (Tyukavina et al. 2022).	[29]Tyukavina Alexandra , Potapov Peter , Hansen Matthew C. , Pickens Amy H. , Stehman Stephen V. , Turubanova Svetlana , Parker Diana , Zalles Viviana , Lima André , Kommareddy Indrani , Song Xiao-Peng , Wang Lei , Harris Nancy; Global Trends of Forest Loss Due to Fire From 2001 to 2019, Frontiers in Remote Sensing.Volume 3 – 2022,2022. [30]Cunningham, C.X., Williamson, G.J. & Bowman, D.M.J.S. Increasing frequency and intensity of the most extreme wildfires on Earth. Nat Ecol Evol 8, 1420–1425 (2024). https://doi.org/10.1038/s41559-024-02452-2 [31]Patrick H. Freeborn, W. Matt Jolly, Mark A. Cochrane, Gareth Roberts, Large wildfire driven increases in nighttime fire activity observed across CONUS from 2003–2020, Remote Sensing of Environment, Volume 268, 2022,12777 [32]Parks, S.A., Guiterman, C.H., Margolis, E.Q. et al. A fire deficit persists across diverse North American forests despite recent increases in area burned. Nat Commun 16, 1493 (2025). https://doi.org/10.1038/s41467-025-56333-8
71	3	The question of forest management and its role on wildfire frequency and intensity is interesting and deserves further study. We agree	
80	1	The global mean sea-level was increasing at a rate of 0.08 inches/yr in 1993 compared 0.18 inches/yr in 2023, almost double the rate[33], which is approximately what is needed to meet the NOAA projections by mid-century and seems plausible. We discuss the NOAA projection for a specific site in the context of high-quality, long-term tide gauge data, but offer no comment on its plausibility. Extrapolating satellite-derived accelerations post 1993 is a perilous business given the well-documented variability of GMSL rates on multi-decadal scales	[33]Hamlington, B.D., Bellas-Manley, A., Willis, J.K. et al. The rate of global sea level rise doubled during the past three decades. Commun Earth Environ 5, 601 (2024). https://doi.org/10.1038/s43247-024-01761-5

88	3	A possible reason the method used by IPCC (optimal fingerprinting) can be considered to address McKitnick criticism is in [34] We have added citation Chen et al. They mostly just reiterate McKitrick's points. They propose a resolution to one part of the problem but it requires very restrictive assumptions.	[34]Chen, H., Chen, S.X. & Mu, M. A statistical review on the optimal fingerprinting approach in climate change studies. Clim Dyn 62, 1439–1446 (2024). https://doi.org/10.1007/s00382-023-06975-5
92	4	The role of clouds remains the single most uncertainty in climate modeling, I am glad this was acknowledged here. Agreed	
93	3	The substance of this paragraph was addressed in my comments in earlier sections. Some of the claims made here are either exaggerations of the usual practice (RCP8.5 being referred to as BAU) or the comment about not fit for purpose (a comment extracted from a publication that is calling out for cloud resolved models. Presumably the reviewer agrees RCP8.5 should not be used as a BAU scenario, so we are in agreement. The Palmer and Stevens paper nonetheless says the models are not currently fit for purpose, an observation echoed by the Nissan et al paper cited.	
105	2,3,4,5	The problem with both the FACE studies and the soybean. Maize and wheat studies cited here are that they don't consider the effect of increase in temperature, changes in precipitation, humidity and other environmental factors along with CO2 increases. These compound effects have been shown in wide number of studies [35] [36][37][38][39]. A meta analysis of the compound effects shows that increase in CO2, along with temperature, precipitation changes effects the quality of the cereal and yields[40]. Another recent study evaluated the impacts on C3 and C4 crops and conclude that CO2 enrichment for C3 crops could increase but will be compensated by temperature increase. CO2 fertilization effect was found to be small for C4 [41]. It is important therefore that we consider the compound effects on crop yields and not just focus on CO2 enrichment. The reviewer is correct that compound effects need to be considered, and indeed one of the problems with a lot of econometric papers on climate change and agriculture is they only look at heat and ignore the CO2 enrichment (Taylor and Schlenker being an important exception). Since our remit was impacts of CO2 emissions a brief discussion of FACE plot experiments and other CO2-only experiments is on topic. We discuss meta-analysis of multivariate (compound) experiments in Section 9.3.	[35] Ruiz-Vera UM, Siebers M, Gray SB, Drag DW, Rosenthal DM, Kimball BA, Ort DR, Bernacchi CJ. Global warming can negate the expected CO2 stimulation in photosynthesis and productivity for soybean grown in the Midwestern United States. Plant Physiol. 2013 May;162(1):410-23 [36] Levine LH, Richards JT, Wheeler RM. Super-elevated CO2 interferes with stomatal response to ABA and night closure in soybean (Glycine max). J Plant Physiol. 2009 Jun 1;166(9):903-13. [37]Večeřová K, Oravec M, Puranik S, Findurová H, Veselá B, Opoku E, Ofori-Amanfo KK, Klem K, Urban O, Sahu PP. Single and interactive effects of variables associated with climate change on wheat metabolome. Front Plant Sci. 2022 Oct 10;13:1002561. [38]Thomas, J.M.G., Boote, K.J., Allen, L.H., Gallo-Meagher, M. and Davis, J.M. (2003), Elevated Temperature and Carbon Dioxide Effects on Soybean Seed Composition and Transcript Abundance. Crop Sci., 43: 1548-1557. https://doi.org/10.2135/cropsci2003.1548 [39] Lesk, C., Anderson, W., Rigden, A. et al. Compound heat and moisture extreme impacts on global crop yields under climate change. Nat Rev Earth Environ 3, 872–889 (2022). https://doi.org/10.1038/s43017-022-00368-8 [40] Ben Mariem S, Soba D, Zhou B, Loladze I, Morales F, Aranjuelo I. Climate Change, Crop Yields, and Grain Quality of C3 Cereals: A Meta-Analysis of [CO2], Temperature, and Drought Effects. Plants. 2021; 10(6):1052. https://doi.org/10.3390/plants10061052 [41]Rezaei, E.E., Webber, H., Asseng, S. et al. Climate change impacts on crop yields. Nat Rev Earth Environ 4, 831–846 (2023). https://doi.org/10.1038/s43017-023-00491-0

106	1	Taylor and Schlenker 2021 also note that 'Climate change will likely have negative impact on agriculture in aggregate, especially in regions exposed to extreme heat. And the CO2-driven yield increases may be offset by effects on food nutrition and quality'. However, the results for US have been a net positive in the recent past but not true in the lower latitudes. Extreme heat may be a net negative, but thus far the overall effects have been positive. Regarding nutrition loss see Section 9.4	
111	1,2	The criticism of NOAA Billion dollar map is likely warranted as it was never completely transparent how the data was generated. However, changing the baseline assumption and refactoring the data using a different set of criteria to make the effects smaller also doesn't make sense. We should continue collecting this data and make the data accessible for researchers and peer review. Figure 10.1 doesn't address these issues, it is another methodology with unknown provenance of the data. We agree that data collection, dissemination and transparency are key. Fig 10.1 is traceable as an indicator though we don't regard it as the final word on the subject either. .	
117	3	Climate change can be expected to increase cold weather extremes in regions that have not been exposed to these effects The reviewer's point is unclear	
118	3	The same Nobel prize winning model in its most recent study concludes that [42] "We conclude with three results relevant for climate policy. First, both current policies (base run) and the extended Paris Accord fall short of limiting global warming to 2 °C or to the cost-benefit optimal level. Second, the economic stakes in global climate policy are substantial, with estimated net present value of economic benefits around \$120 trillion from the cost-benefit optimal policy. Third, once differences in discounting are considered, the baseline DICE-2023 estimate of the social cost of carbon (\$66/tCO₂ for 2020) aligns closely with other recent estimates". Obviously this is the latest and the current version of the model and these results are quite different from thus discussed on this page. These conclusions should also be included in this discussion to avoid selective reading of the literature. We discuss Barrage and Nordhaus (2024) on p. 121.	[42]L. Barrage, & W. Nordhaus, Policies, projections, and the social cost of carbon: Results from the DICE-2023 model, Proc. Natl. Acad. Sci. U.S.A. 121 (13) e2312030121, https://doi.org/10.1073/pnas.2312030121 (2024).

120	1	Similar empirical studies [43] point to "Using an empirical approach that provides a robust lower bound on the persistence of impacts on economic growth, we find that the world economy is committed to an income reduction of 19% within the next 26 years independent of future emission choices (relative to a baseline without climate impacts, likely range of 11–29% accounting for physical climate and empirical uncertainty). These damages already outweigh the mitigation costs required to limit global warming to 2 °C by sixfold over this near-term time frame and thereafter diverge strongly dependent on emission choices. Committed damages arise predominantly through changes in average temperature, but accounting for further climatic components raises estimates by approximately 50% and leads to stronger regional heterogeneity. Committed losses are projected for all regions except those at very high latitudes, at which reductions in temperature variability bring benefits. The largest losses are committed at lower latitudes in regions with lower cumulative historical emissions and lower present-day income". This study uses more of the daily temperature precipitation from 1600 hundred locations world wide instead of global means. There are numerous studies that show the cost, many described in the COP 21 and follow-on meetings The Kotz et al. paper is subject to a cautionary notice at the Nature website that “the reliability of data and methodology presented in this manuscript is currently in question.” https://www.nature.com/articles/s41586-024-07219-0 The literature showing growth effects of temperature increase is pretty robust. And though we don’t cite it, there is a new paper just accepted at Climate Change Economics by McKittrick and Devina Lakhtakia adding to the evidence using 2 newly-constructed global data bases.	[43] Kotz, M., Levermann, A. & Wenz, L. The economic commitment of climate change. Nature 628, 551–557 (2024). https://doi.org/10.1038/s41586-024-07219-0
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Page	para	Comment or Suggested Edit
		The report indicates numerous examples of uncertainties in climate science. It would be extremely helpful if these undertainties were made more explicit, listed in a concluding section, and prioritized in some way - what are the top 10 priority research areas? If the opening remarks by Secretary Wright and the title pointed to uncertainties and the need for more research, I believe this report will be better received. As I read the report and left review comments below, I bolded items where I gather the authors imply that research is needed. Consider including these kinds of statements throughout and adding a summary. An interesting suggestion but this must be deferred to a separate report.
viii	3	This would be more correct: "Some scenarios of future CO2 emissions overestimate observed levels" Sentence has been removed
viii	5	This would be more correct: Some detailed climate models run “hot” in their simulation of the climate of the past few decades, while many have underestimated the amount of surface warming. More research is needed to select the most skillful and reliable models. The need for more research on model development is implicit. Again, this would be a good topic for a separate report.
viii	6	Need reference for: "Claims of increased frequency or intensity of hurricanes, tornadoes, floods, and droughts are not supported by historical data." More research is needed to identify how frequency and intensity of weather events are affected by environmental conditions. The ES statements are all supported by the document chapters We have added reference to the specific sections in the revised summary.
4ff		It is misleading to separate the photo-chemical effects of CO2 from the temperature effects. Extreme heat is hard on plants, causing them to lose water, close stomata, etc. It depends on the plant and the degree of heat. More research is needed to understand how the temperature (mean and variability) affects plant productivity and resilience. 1st paragraph of Section 2.1, Added sentence “Here we focus just on CO₂ fertilization; research on combined effects due to temperature and precipitation changes are discussed in Chapter 10.”
7		Again it is misleading to separate the CO2 and the temperature effects on the oceans. The warming surface waters is having significant impact on ocean fish and habitats due to the overall increase in global temperatures This section is specifically on changing alkalinity. We haven’t included a review of temperature effects on fish populations: it is one of the impacts topics we were not able to survey.
14	4	More accurate to say "because their effect remain largely unknown". Good examples include natural aerosols (sea salt, dust, biogenic aerosols). Are there others to mention? More research is needed to understand radiative effects of both natural and anthropogenic sources of radiative variability throughout the atmospheric column. Sentence added: “Natural sources of global energy imbalance other than volcanoes and total solar irradiance (TSI) are not included in these graphs because their effects remain largely unknown.”
17	figure	What scenarios are the models using in this figure? SRES, as stated in text, also others including Hansen A,B,C and others as described in the Hausfather et al. paper.
19		Vegetation is only one way that CO2 is accommodated by land processes, the other is through soils and microbial processes Microbial dynamics is complex and challenging to understand but may also be a way to boost CO2 absorption by soils (which also enriches the soil nutrients!). More research is needed to better understand how plants and soil systems remove CO2 from the atmosphere.

		Noted.
19	5	More research is needed to improve the understanding and representation of land systems, carbon and nutrient fluxes in models. Noted.
20		Carbon uptake by oceans and ocean ecosystems is less well studied and modeled. The lack of spread of carbon in ocean models may result from insufficient information, ocean biogeochemistry, ecosystems and model development. More research is needed to study and model carbon, nutrient and ecosystem processes and dynamics in ocean models. Noted.
		More research is needed to map warming trends in urban, non-urban and other environments and to ensure accurate representation of regional and global trends in temperature. Noted.
27	2	"This practice" - need to clarify whether this refers to "emergent property" or discarding. I believe the authors mean the latter, but it should be made more clear Sentence rewritten: "Parameter tuning to yield an ECS target was commonplace for the models used in AR4; modelers have moved away from this practice with time."
27	3	"assumed positive cloud feedback" - this is an emergent property too, based on simulation/observation. Should be rephrased to not use the word "assumed". Further research is needed to better understand and model how clouds change with temperature and what the feedback effects are on the radiative balance. We have deleted the word "assumed".
		Further research is needed to narrow the uncertainty on climate sensitivity, both using models and observations. Noted.
33		The text and figure refer to 33 models, which would include the best and worst. How much does the uncertainty/range change for a subset of best-performing models? The modeling community doesn't have a leaderboard of best-to-worst models. Some of the "prestige" models from the US and UK have very high sensitivities, but they are preferred by many users for particular applications.
34	figure	How many models are included in each case? High ECS 14, Medium ECS 11, Low ECS 13. Added to Figure caption.
36	figure 5.4	How are the lower and mid-troposphere defined? Standard definitions in the paper: LT surface to ~9km, MT surface to ~15 km. Added in the Fig caption.
37	4	"excess amplification" - amplification of what? And Figure 5.6 shows tropics, is this bias only tropical? Sentence changed: "Another important model-observational discrepancy is the excess amplification or warming with altitude..." The bias is only observed in the tropics but the amplification pattern is only predicted in the tropics. Also bear in mind the tropics is nearly half the atmosphere and is where most solar energy enters the climate system.
38		Further research is needed to improve how models represent clouds and dynamics; additional observations are also needed to improve these representations.

		Noted.
42		Further research is needed to improve how models represent planetary albedo to align with observations. Noted.
		The greater concern with hurricanes is the amount of moisture they carry and drop after landfall, i.e. flooding. While this is an active area of research, it is worth noting that this may be the bigger issue for hurricane change with warming This is covered in the AR6 assessment as quoted, as it's one of the intensity metrics. Currently too little data to assess for trends.
56-57		The concern for many are the extreme hot temperatures without night-time cooling that provides an opportunity for recovery. This section doesn't address this. In this regards, the focus on the difference between extreme hot and extreme cold is misleading when generalizing about decrease in extremes (figure 6.3.4) - the decrease is because we don't have as much extreme cold. This metric is covered by the data in Sct 6.3.3.
60		Combining heat and humidity as a measure of extreme heat is not unusual since it is this combination that is most impactful to human health - the body cannot cool if the humidity and heat are both high. Since a warmer atmosphere holds more moisture, and CO2 causes heat (and therefore moisture) to increase, this combination is a relevant assessment of the effects of CO2 on extreme conditions that make it difficult for people. Also combining with urban regions is helpful since that is where most people live. These points are not made in the report and are important. <u>S</u> entence be changed to read: "Given the heatwave definition and urban focus, while these increasing values since 1960 point to a growing human impact they are not informative about long term trends or..." Added sentence at end of paragraph (Irrespective of the ultimate cause of regional trends, heatwaves have important effects on society which must be addressed, as we discuss in Chapter 10.), which should address reviewer concerns.
		Patterns and variability of precipitation are poorly understood and models are particularly challenged to represent and predict precipitation, further research in this area is needed Noted.
		The authors rightly describe the extreme uncertainty regarding sea level rise. It would be helpful to add some information about the complex, highly nonlinear and uncertain processes involved with ice-sheet dynamics and ice-sheet - ocean interactions that are especially key for Antarctica. More research is needed to study and model ice sheet dynamics and ice-sheet ocean interactions. Noted. Canvassing ice sheet and ocean dynamics in that much detail beyond our scope but would belong in a separate report.
84	3	Further research is needed on climate variability, effects of ocean circulation, solar variability, model simulation of variability and statistical methods. Noted.
87-88		Anthropogenic aerosol effects may also influence the inflections in temperature in ways that are challenging to account for Noted.
91	2 and 3	Reductions in aerosol indirect (cloud brightening) effects may also explain the reduced planetary albedo, not just from ships but from reduced fossil fuel aerosols coming from the continents, China and east asia especially. This paper describes the reduction in aerosols https://www.nature.com/articles/s41612-023-00488-y This is another area that needs further research! Noted.
94	table 8.1	What are the numbers in the 3rd column? Please include in the caption. Good point. Added to caption: "Numbers refer to footnotes in original source that specify locations or region-specific confidence levels."

95	2	I don't see an inconsistency between what Ch 11 and Ch 8 say. The first is about precipitation, droughts, tropical cyclones, heat - overall. The 2nd (ch 8) is about specific types of events in specific regions. Suggest to soften the statement about the contrasting assertions. Sentences at issue are “Evidence of observed changes in extremes and their attribution to human influence (including greenhouse gas and aerosol emissions and land-use changes) has strengthened since AR5, in particular for extreme precipitation, droughts, tropical cyclones and compound extremes” versus “No evidence of emergence in the historical period of a change in river floods, heavy precipitation, drought, fire weather, severe wind storms, and tropical cyclones”. these statements are manifestly in conflict.
		The science of attribution of extreme events is challenging and computationally expensive; this is an area where further research is needed with potential to use AI to improve statistics Noted. Not sure about the computationally intensive point: many just run univariate GEV models and overinterpret the results. AI won't fix the bad methodology.
104		This section seems to gloss over many key points. Certainly there are limits where extreme heat and drought (water is key) could be detrimental to crops. It also depends on which crops. There is extensive literature on how certain crop viability is shifting. So a region with particular crop experience and cultivation may no longer be able to sustain that agriculture. There is cost to shifting crops across regions and farmers that needs to be accounted for. And water is key - is there enough water in various regions for energy (power plants need water) as well as agriculture when conditions are very hot for sustained periods? The comment seems to be about the validity of the underlying literature rather than our summary of it. The econometric literature tries to synthesize patterns over large areas to get at overall trends, rather than the agronomy focus on specific sites and crops.
111		Severe weather continues to disproportionately impact lower income communities as well as business that requires outdoor exposure. A thorough analysis would need to include these points and I recommend adding this caveat to the section. This is specifically addressed in Section 10.3.2
		Related to my previous comment, deaths due to extreme heat conditions appear to be increasing, particularly for certain populations and for those with underlying health conditions (which can be hard to detangle when attributing mortality). For example https://jamanetwork.com/journals/jamanetworkopen/fullarticle/2792389 Again this topic is specifically addressed in Section 10.3.2
114		An additional concern is if the grid fails under either extreme heat demands or cold/severe-storm events, the impact on communities will not be uniform and depends on grid infrastructure which can be better in more affluent communities Good point. We don't discuss what makes for grid stability versus instability, especially in the context of many jurisdictions closing reliable power plants and opting for intermittent renewables, but the correlation between extreme weather and the likelihood of power failure is an important topic.
		The impacts of sea level rise and coastal inundation on coastal communities and in low-lying countries should be addressed. This will have impacts particularly on certain countries/regions but also on world economics Good point. The topic is germane but runs into the limits of what we can cover in our limited time. To some extent it is covered in the economics chapter since IAM damage functions include effects of sea level rise on coastal areas.
		Research is needed to better understood the likelihood and impacts of various tipping points, including changes to ocean circulation patterns, ice sheet acceleration, sea-ice loss, die-off of tropical forests, thawing of permafrost, etc. Noted. Hopefully our discussion clarifies the different types of “tipping point” topics.

		Do the authors think it worthwhile for private or public research, such as at DOE, to seek breakthrough technology that might effectively remove massive amounts of CO2 from the atmosphere? No comment
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Chapter Number	Page Number	Approximate Paragraph Number	Comment or Suggested Edit
	i		In general, criticism and re-analysis of past research is always welcome. This report summarizes some substantial concerns from a small group of researchers. The report should be reframed to reflect this and I provide some suggestions in a later comment. I suspect that simply publishing this report will not offset the hundreds of reports and publications on CO2 and Climate that have come before it. But, new analyses and communication within and outside the research community could make a positive difference, should the findings in this report be substantiated. The DOE Office of Science BER could be provided an opportunity to address the research uncertainties in this document in a thorough and robust manner. Noted.
	iii	1	As someone who used to read the DOE reports on CO2 as a graduate student in the 1990s and then conducted numerous analyses of carbon on many topic areas which were later confirmed by numerous other analyses, it seems somewhat cavalier for DOE to issue one brief conclusive report on the "impacts of carbon dioxide emissions". I suggest the following option: Change the title to "Topics of Uncertainty on the Impacts of Carbon Dioxide Emissions on the U.S. Climate" or " An Assessment of CO2 Impacts on Climate to Guide Future Research ", and then use this report to drive research/analyses for each report chapter which would conclude in separate topical reports that could be finished within a year or two. In doing so, the DOE would positively contribute to addressing some of these standing issues and hopefully enable the nation to move forward in a more productive manner. Noted. New title: "A Critical Assessment of the Impacts of Carbon Dioxide Emissions on U.S. Climate."
	vii	Forward	I appreciate the perspective. It could be useful to add or edit the Secretary's Forward to indicate that "the report has been purposefully commissioned to researchers with alternative perspectives in an effort to address these perspectives and provide some closure for the American public, the US Economy, and US energy policy." I suggest this, because it will be very obvious that a handful of skeptics were chosen out of literally hundreds of extremely active and well-respected scientist. Addressing that upfront will immediately reduce criticism, and hopefully generate a positive environment to address these issues. Everyone would like to see convergence of findings on these issues. Noted. The suggested sentence is not well-phrased, but the reviewer is correct that everyone will see the names and know that the document is coming from a particular perspective. The problem is no such concerns were expressed when people with known biases got put in charge of NCA or IPCC chapters. The path to convergence starts with the wider community admitting that they've been keeping a lid on the necessary discussions. In any event, we do not have any control over the Secretary's Foreword
	viii	2	Any increase in greening from additional CO2 will likely be more than offset by longer growing seasons with higher temperatures (i.e., more days over 100). This causes serious risk to the US midwestern "bread basket", which is why this has been considered a national security concern. The data supporting this trend has been published a few times now in Science by a fellow out of Stanford, among others. Sources: Lobell and Field. 2007, doi: 10.1088/1748-9326/2/1/014002; also Sloat et al. 2020, doi:10.1038/s41467-020-15076-4 has a thorough introduction with many citations. The effects, including compound effects, on Agriculture are addressed in Ch 9

	viii	3	My understanding is that the upper bound of emissions (i.e., very high) is business as usual, with no mitigation, and a high-end of growth. It's a worst, yet plausible, case scenario. Similarly, the lowest emissions (i.e., very low) scenario includes technologies that don't yet exist including significant CO2 removal. Both are there to understand plausible extremes. The more likely scenarios are the low to high scenarios. I'm sure researchers are open to revisiting how things are bounded and presented if there are reasonable objections. It seems that within the expert community people are quick to set RCP8.5 aside as a fringe scenario. The problem is it dominates the impacts literature and the media coverage thereof. So the critical discussion herein is needed.
	viii	4	This may be true. But data driven analyses may not capture anticipated feedbacks. They attempt to by design. Limited by data especially ocean heat content, but they take it into account.
	viii	6	I am a big fan of data and meta-analysis. However, I'll note that I do not think forest management practices are overlooked. In fact, it is one of the main driving story lines (i.e., too much fuelwood exists, so we need to harvest more forest). There remains strong debate on the efficacy of fuelwood removal over large areas as an effective means of wildfire reduction. The key to this analysis is to look at whether the extent (not the numbers) of fires has grown and whether the burned areas are returning to forests or returning to brush or grassland. If it's the latter, then that is more likely due to regional changes in climate (temp and precipitation). Again the issue may be what experts discuss among themselves versus what's out there publicly.
	viii	8	I think we can point to sea-level rise in south florida, crop damage, and disease vectors fairly easily. I'm not aware of what costs are associated with these 3 items, but I suspect it's quite large. Dealt with in Ch 11 on economic costs and the SCC.
	viii	9	Yes, this is an important paragraph. Most scientists are not activists. Just because there is a science finding, scientists don't expect that to turn into policy. The public and the democratic system can determine the policy using an evidence-based risk/benefit analysis, just as noted here. Opening up this conversation (which should have been done decades ago) would be welcome and would better engage the public on this issue. DOE could in fact take a leading role here and help address a major national issue. A point of sincere agreement.
1	2		This is all generally correct. I would like to think this is common knowledge. Yes but bears repetition
2	4	1	Why use studies based on satellite sensors? I'm quite sure there are studies (again, out of Stanford) that use historic yield data (county-scale USDA data and global FAO data) with associated temperature and precipitation data. Those analyses do not agree with your findings here, I think. But, we can certainly pose these questions to our scientists that study and develop land models and develop a report based on all available data. I'll also note that for sometime data synthesis wasn't considered fundamental science and was therefore largely ignored. Enabling the researchers to conduct robust data analyses on this and other topics would be largely welcomed. See Ch 9 on Ag effects for the studies on yield data.
2	5	Figure 2.2	All data is interesting and useful for perspective and understanding. But isn't the main question whether we are driving up CO2 and temp at a rapid pace in the post-industrial era? This graph does not help answer that question. Figure 2.2 has been deleted.

2	7	3	Stating that increased aridity in drylands will not lead to decreased productivity seems a bit questionable. We can point to precipitation gradients from high to low precipitation that supports forests to grasslands to shrublands, respectively. Less water in general will mean less productivity; this can be seen in different forest compositions as well. The authors cite Zhang et al. (2014), but Zhang's main point is that the direct CO2 effect may be causing greening in drylands that offsets the decreased precipitation. This may be more specific to dryland vegetation, and Zhang et al. specifically state that this finding is relevant to north-east Brazil, Namibia, western Sahel, Horn of Africa, and central Asia. Zhang 2024, not 2014. The point here is to explain benefits of higher CO2 in terms of crop water productivity. Zhang acknowledges increase in desertification (as we note) but points to a global pattern of greening in arid areas.
2	9	3	I do not disagree with this observation of a decline effect, and I have seen it in some of the early biomass energy analyses that appeared in prominent journals and were then corrected over a few years. While it is unfortunate that some try to grab headlines with unsupported assumptions, it does seem that other researchers tend to provide counter arguments and analyses fairly quickly, indicating that the system does in fact work fairly well. Yes, part of the problem is the later counter-evidence gets relatively little attention, hence this report.
3	19	last	Yes, CO2 uptake by land processes have uncertainty associated with them. It's worth noting that DOE researchers (many of whom are retired now) were world-leading and making progress in this area, but nearly all work on CO2 uptake and emissions stopped 10-15 years ago due to political pressure and political concerns. The Carbon Dioxide Information and Analysis Center at ORNL, now cancelled, was a main contributor to the Global Carbon Project. Now the US is woefully behind and ceding expertise on this to the Europeans (as cited in this report). We are also ceding expertise on carbon monitoring via satellites to other countries. There's nothing necessarily wrong with this if this is where the US wants to go, but it's important to recognize that we have lost significant capacity due to policy changes over many years. We specifically recommend the government increase its focus on basic climate monitoring and data dissemination in the Concluding Thoughts section. The CDIAC was very valuable and we lament its closure.
5	44	1	Some of the early work on this topic was done by researchers at Stanford. But there are plenty of other papers. Here's one that came up in a quick search: https://www.pnas.org/doi/10.1073/pnas.0906865106 We discuss this paper in Ch 9.
5	44	1	Contradicting a number of large meta-analyses on the topic of decreased yields with increased temperature is ok. That's what science is all about. But it's going to have to be done in a more robust way than a couple paragraphs here with one or two citations. This section concerns temperature trends in the corn belt; the discussion of yields is in Ch 9.
6	47	chapter summary	This is very important. I have not dug into the data on extreme storm data (both numbers and intensity). Getting researchers together that disagree on a topic, analyze the data, and publish findings together can be very productive. DOE could sponsor a workshop and bring together a broad range of experts and find answers to extreme weather trends. Good idea

6	47	chapter summary	I conducted some analyses on wildfire years ago. I saw the bias in the university textbooks and in the wildfire working groups, who essentially went by textbook teachings. It was rare that people went back to the original data. The last time I checked the actual data (a good 10 years ago), I found that...you're correct...the number of fires had not changed much. What does seem to be changing is that the extent of fires has grown, and this is mostly attributable to hotter and drier conditions. I do not believe there is more fuel load now than there was before the US was settled. Arguing that harvesting will decrease fire is nonsensical; you have no idea where the lightning is going to strike and subsidizing the thinning of all forest in the western fire zone would be cost prohibitive. I think we need to stop using wildfire management as a justification for harvesting. If we want to harvest more, than do it, and stop using fuel load arguments (which is more of a concept than proven reality) as a reason to do it. The biggest issue here is encroachment of housing communities into the Wild-Urban Interface (WUI) and how we want to handle that with policies. Fire is really only bad if it's burning someone's house down. Otherwise, it's a natural progression in forest dynamics. Sources: Campbell et al. 2012. doi:10.1890/110057.; Schoennagel and Nelson. 2011. doi:10.1890/090199. Excellent comments. It's a big topic we can only glance at but we are trying just to get some pertinent data into the discussion.
6	57	6.3.2	The NCA5 is cited here and elsewhere. However, the USGCRP website that hosts the NCA reports has been removed. Hence, there is no way to read and review the referenced findings. This DOE report is therefore arguing against something that no longer exists, and that may appear questionable. Noted. Out of our control.
7	74	2	Given the satellite observed ice sheet dynamics, I'm not sure that ice sheet retreat is that hypothetical. Our reference is to hypothetical instabilities, i.e. massive disintegration.
7	all		This is the first time I have seen all sea-level rise estimates discredited by an offset from land subsidence. This is certainly a global dynamic that we would want to capture and understand in Earth system models, and DOE can look into this further with analyses and experiments if necessary. Noted.
8	86	all	This and other chapters cite the IPCC AR6. The IPCC provides a global process to address the issues and uncertainties in this report. Perhaps this report can set a path for pivoting research and re-engaging in a useful manner. That's a big ask.
9	104	chapter summary	The last sentence in this summary is not necessarily accurate. The authors cite Ainsworth and Long (2020) with respect to increased CO2 effects on crops. But, Ainsworth and Long (2020) indicate that increased temp and decreased precipitation will have negative impacts on crops, while increased CO2 may offset some of those negative effects. It is the impact of CO2 on climate and the subsequent impact on crops that is the concern; not the direct effect of CO2 on crops. Rewritten: "There is reason to conclude that on balance climate change has been and will continue to be neutral or beneficial for most U.S. agriculture

Chapter Number	Page Number	Approximate Paragraph Number	Comment or Suggested Edit
2	3	2.1.1, para. 2	Zheng not in reference list. Do you mean Zeng? Yes, fixed.
2	3	2.1.1, para. 3	"nearly double"? Why exaggerate? - it's a 76% increase. And they reported that future increase in GPP in extra-tropical northern hemisphere (e.g., U.S.) for [CO2] from 300 to 600 ppm of 42%, or 32% more than previous predictions (not 76%). Replace "nearly double" with "much larger than"
2	3	2.1.1, para.1	Zhu also explained that "other factors (OF) not considered in models, such as forest management, grazing, changes in cultivation practices and varieties, irrigation and disturbances such as storms and insect attacks, can be a cause of mismatch between observed and simulated LAI trends.... OF contributes the most to the observed LAI trend over 25.0% (increase) and 5.3% (decrease) of the vegetated area" We don't claim CO2 is the only factor.
2	3	2.1.1, para.2	This report is meant to focus on the U.S., so it would be more appropriate to discuss greening trends in the U.S. Fig. 2.1 shows that the trend in LAI across much of the U.S., with the exception of the Southeast, is less than the global average. Negative OF effects are mainly found in northern high latitudes (e.g., Alaska). Growing season warmth is the dominant factor in cold regions (10.1038/s41558-018-0258-y). Significant browning also is occurring in parts of Alaska (10.1175/EI-D-20-0025.1) But the effect is still positive. In Ch 9 we discuss US-specific evidence concerning CO2 and crop yields.
2	3	2.1.1, para.2	Greening of the Arctic can have important adverse consequences. Direct observations support that warming and increased vegetation cover in the Boreal-Arctic region showed a "robust increasing trend of CH4 emissions (+8.9%) with strong inter-annual variability. The majority of emission increases occurred in early summer (June and July) and were mainly driven by warming (52.3%) and ecosystem productivity (40.7%)." Yuan et al 2024 (10.1038/s41558-024-01933-3). We don't claim it's good or bad, only that it's been observed.
2	3	2.1.1, para.3	Wang et al 2020 (10.1126/science.abb7772) report a decline over recent decades in CO2 effects on global photosynthesis, possibly due to increasing nutrient interactions or interactions with effects on LAI. It's a controversial finding that attracted skeptical comments in <i>Science</i> regarding the validity of the satellite data and the statistical methods used https://www.science.org/doi/10.1126/science.abg2947 https://www.science.org/doi/10.1126/science.abg4420 https://www.science.org/doi/10.1126/science.abg5673 While the authors in reply defend the satellite data, they acknowledge they may not have dealt adequately with confounding non-CO2 factors. So, it seems premature to invoke this result.

2	4	2.1.2	Much of this information in this section is appropriately covered in chapter 9 on agriculture. Chapter 2 purports to have a wider focus (global and environment responses), so it should not dwell on responses of individual crop plants (crops account for only 15% of global GPP). There has been plenty written on ecosystem response to eCO₂, but evidence such as Fig. 2.3 is not relevant. All analyses of direct eCO₂ effects reveal an overriding interaction with other environmental factors (nitrogen and temperature), and global models show that eCO₂ only partially compensates for the negative effects of climate change on ecosystem carbon response. We struggled with how to divide material up in Ch 2 and Ch 9. The solution we came up with was that here we are addressing direct CO₂ effects, namely CO₂ fertilization; later we discuss Ag effects in more detail. Not everyone paid attention in high school biology. Figure 2.3 serves as an illustration of a point that non-specialists are often surprised to learn, namely that CO₂ is plant food.
2	4	2.1.1, last para.	Zhang, 2025 (10.1016/j.ecolind.2025.113762) reported that although vegetation cover was increasing, resilience against external disturbances (drought, heatwaves, wildfires) has simultaneously declined. They use the lag-1 autocorrelation coefficient as the measure of resilience based on Critical Slowing Down theory. The results are suggestive but very noisy and require a lot of assumptions to equate to a loss of ecosystem resilience. Since the authors also show that vegetation cover is increasing at the same time the results don't appear strong enough to merit any change in our discussion.
2	7	2.1.3, para. 2	Cheng did NOT report "increased plant growth"; they reported increased GPP. The distinction is fundamentally important, has been recognized for decades, and is completely ignored in this chapter. Cheng et al. did refer to an increase in Net Primary Production as well, but you are correct they focused on Gross Primary Production. The sentence has been rephrased "Similarly, Cheng <i>et al.</i> (2017) noted that increased Gross Primary Production from 1982 to 2011 due to rising..." and GPP has been added to the Glossary. More generally since GPP and NPP are closely correlated the use of "plant growth" is suitable.
2	6-7	2.1.3	This material is repeated in chapter 9 where it belongs. Here, it would be more appropriate to focus on how eCO₂ interacts with drought on ecosystem response (e.g., 10.3390/atmos12020212) We tried to avoid repetition but there is some. We discuss drought in Ch 8: since we don't see global and US trends, the issue of declining CO₂ fertilization during droughts isn't a key issue at this point.
3	19	3.2.2, last para.	Regarding the uncertainty in the land sink, a primary reason for this uncertainty is that the primary response of photosynthesis to CO₂ does not predict C sequestration because of strong interactions between CO₂ and climate, nutrients, stand dynamics, and other factors. That is why the almost complete focus in this report on crop plants and plant responses in greenhouses does not inform us about "CO₂ uptake by land processes" We aren't making any strong claims about the land sink, we only report the results in the Global Carbon Project. Not sure if the reviewer is challenging them or not.
9	104	all	This chapter is silent on agricultural effects on atmosphere and climate. Sokal and Kachel 2025 (10.3390/en18092272) conclude: "The current study demonstrated that agricultural activities significantly contribute to the emissions of three main greenhouse gases: carbon dioxide, nitrous oxide, and methane." Emissions from the Ag sector are included in the IAM results discussed in Ch 11.

9	105	9.2, para. 2	The section title is "Field studies..." but the next three paragraphs are primarily about responses of potted plants in growth chambers or greenhouses (with exception of Blandino, which was a field study and showed a much smaller response than the pot studies). There is an important reason that the field data cited in Blandino and in SoyFACE show a smaller response than data from the potted plants under artificial conditions. These data are barely relevant to the topic you are trying to address. FACE plot studies are field studies, and we also discuss other field studies. However, we also include some lab experiments. <u>Heading changed to "Field and laboratory studies of CO2 enrichment."</u>
9	105	9.2, para. 3, 4, 5	CO2Science.org website is a highly biased and unreliable website. They report on papers, or specific sentences in papers, that support their view and ignore conflicting reports or qualifying sentences. If there was another site that gathered that much data, we'd cite it. We'd need some evidence of actual bias to disqualify the CO2Science.org archive given the extent of its coverage.
9	105	9.2, para. 3	This is a report for the US. Why are you citing soybean studies in China when there is a major soyFACE study in Illinois? Yield in SoyFACE increased in eCO2 but declined with increasing temperature (10.1093/jxb/erac503). eCO2 did not result in water savings as hypothesized but rather increased soil moisture depletion. (10.1098/rstb.2024.0230) The Chinese study is on point regarding CO2 enrichment. The first study referenced by the reviewer (10.1093/jxb/erac503) only subjected soybean to elevated temperature without varying CO2, so it isn't relevant to our discussion. The second reference is to a general literature survey which doesn't appear to say anything different from what our chapter says.
9	106	9.2, para. 2	Ainsworth et al, 2025 (10.1093/jxb/erac503) state: "Early season stimulation in biomass at elevated [CO2] more than offset lower stomatal conductance in crops, resulting in greater depletion of soil moisture, instead of water savings [30,31]. Wheat tiller survival and grain filling was also reduced by elevated [CO2] under dry conditions [32]. The CO2 fertilization effect was also significantly reduced by growth at elevated temperatures in FACE experiments, again suggesting that high atmospheric [CO2] may not offset the negative impacts of global warming [29]. Thus, it may be that, to date, elevated [CO2] and the resultant climate changes have had net positive effects on agroecosystem productivity, but as climate change intensifies, there will be a shift towards net negative effects as has been suggested for natural ecosystems [3]." Our summary statement on the Ainsworth review is accurate regarding the CO2 effect. They raise the question of compound effects from simultaneous changes in CO2 and temperature. That topic is taken up in the discussion of the NBER study and Section 9.3

9	107	1st para.	Another study that should be cited is Hultgren et al, 2025 (10.1038/s41586-025-09085-w). "For all crops except rice, we estimate that warming will likely reduce global yields by 2050 (probability of loss ranges from 0.701 (sorghum) to 0.946 (wheat)... " "We estimate that global production declines 5.5×10^{14} kcal annually per 1 °C global mean surface temperature (GMST) rise (120 kcal per person per day or 4.4% of recommended consumption per 1 °C; $P < 0.001$). We project that adaptation and income growth alleviate 23% of global losses in 2050 and 34% at the end of the century (6% and 12%, respectively; moderate-emissions scenario), but substantial residual losses remain for all staples except rice." Regions with moderate average temperatures tend to suffer the largest yield losses. "Adjusting for CO2 fertilization does not qualitatively alter the structure of our findings ... but it does reduce the central estimate for end-of-century yield losses by 5.0– 9.5 percentage points... and increases the likelihood of positive aggregate effects." The Hultgren study came out after our draft had been complete. The headline results which generated massive publicity were based on RCP8.5 with no CO2 fertilization. One of our other reviewers wondered why we focused so much on the deficiencies of RCP8.5 when everyone knows it's a marginal case – this study is a good illustration of why. Fig 3 in Hultgren
9	108	1st	Reference to Ziska paper greatly mischaracterizes it. Ziska clearly indicates an effect of eCO2 on nutritional quality, but he indicates there is uncertainty in some of the mechanisms behind that response, including the simple dilution hypothesis. He states up front: "However, it is the direct effect of rising [CO2] on plant chemistry and stoichiometry that represents a continuing threat to the nutritional integrity of both human and animal systems." Ziska doesn't dismiss CO2, but he points out that nutrient dilution has also been observed under historical conditions in which CO2 was likely not the driver. Sentence rephrased "as is the evidence that nutrient dilution observed to date is entirely attributable to higher CO₂"
9	108	9.4, para. 1	Evidence for nutritional decline under eCO2 is widespread: Toreti et al, 2020 10.1038/s43016-020-00195-4) "A meta-analysis of 228 pairs of experimental observations on barley, potato, rice and wheat reported reductions in protein concentrations ranging on average from –15.3% to –9.8% under eCO2, while the reduction was relatively small (–1.4%) in soybean." AND Ainsworth et al 2025: "The positive 'CO2 fertilization' effect on crop yield is complicated by a decrease in many nutrients in elevated [CO2]. An analysis of 130 varieties of plants found that elevated [CO2] decreased the concentration of 25 important minerals by 8% on average, and the carbohydrate to mineral ratio was increased in these plants." We don't dispute any of this; it corresponds with what we say.
9	109		URL for Taylor and Schlenker paper is incorrect. It should be https://www.nber.org/papers/w29320. "papers" not "paper. Was this report peer reviewed? I cannot find it in a peer-reviewed journal. Thanks for the correction We don't think it has appeared in print anywhere other than the NBER series.

12	129	This chapter is highly distressing, but I suppose it encapsulates the whole thesis of the document. Apparently, there is no point in policies to reduce emissions (or make the necessary changes in energy policy to accomplish emission reductions) because it will make no difference to the atmospheric CO2 concentration or climate. So why bother? Would you have said this in 1970 (CO2= 326), 1990 (CO2 = 354 ppm), or 2010 (CO2=390)? Probably so. And in 30 years when CO2 is 500 ppm, will it be fair to look back and evaluate your apparent call for inaction in the statement here "And even if global emissions were to stop tomorrow, it would take decades or centuries to see a meaningful reduction in the global CO2 concentration and hence human influences on the climate." But I suppose this attitude is justified if there is no adverse consequence and the only important effect of rising CO2 is to make little plants in greenhouse grow bigger (e.g., Fig. 2.3), and indirect effects on heat stress, drought, fires, sea level, etc. aren't worth discussing. The chapter rehearses some straightforward quantitative aspects of mitigation without making any policy recommendations. The reviewer is calling attention to actual problems finding mitigation strategies with positive net benefits.
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Ch	Page	para	Comment or Suggested Edit
1	2	full text	This chapter points out that the primary concern around human emissions of CO2 is that it changes the radiative balance of the atmosphere. The chapter also makes the points that CO2 has no toxicological effects on humans at current atmospheric levels, and that CO2 is necessary for photosynthesis, with the potential to stimulate plant growth at higher concentrations. These points are well-established scientific facts. Yes and experts know them but non-experts aren't necessarily aware of them.
2	3	3	It would be more reflective of the literature to say here that "Many studies over the past decade have confirmed a global greening pattern (increase in LAI) attributable to rising CO2 levels, changes in climate, changes in nitrogen deposition, land cover change, and other factors." We have added "in part" to sentence.
2	3	3	While the Zhu et al. 2016 study did find that 70.1% (+/- 29.4%) of the integrated global change in LAI was attributable to changes in CO2, most of the observed change in LAI is concentrated in the tropical forest regions of South America, Africa, and Indonesia, as shown in this report's Figure 2.1, reproduced from Zhu et al. In terms of the fraction of global land area influenced most by the factors considered in Zhu et al. 2016, effects on LAI from changing climate is the dominant factor (positive effects for 28.4% of land area plus negative effects on 4.7%, for a total area with dominant climate influence of 33.1%), followed by other factors not represented explicitly in the study or in the models (positive effects on LAI from other factors over 25.0% of land area plus negative effects over 5.3%, for a total area with other factors as the dominant effect of 30.3%), and then CO2 effects (23.2% of land area dominated by positive effects and 0.1% with negative effects, for a total of 23.4%). Since the report is focused on the U.S. it would be helpful to indicate the global context for the numbers reported. We have added new text "Other contributors included land-use changes, warming and nitrogen. The fraction attributable to CO2 was largest in the tropics; other factors played more dominant roles in CONUS."
2	3	3	The dominant factors related to changes in LAI over the U.S. (including Alaska) are land cover change (+LCC), climate effects (+CLI), other factors (+OF and -OF), and then CO2 (+CO2). There are more extensive regions where +CO2 is the dominant factor for the boreal forest zone in Canada. Given that, and given the explicit focus of the report on the U.S., it would be useful to call out in this section that the global greening pattern has distinct geographic variation in driving factors, and that over the target geography of the U.S. it seems to be dominated by factors other than increasing CO2. See my powerpoint slide 1 for a zoom on the area of interest from the Zhu et al. 2016 paper. Addressed with new sentence: CO2 fertilization effects are influenced by local temperature and nutrient and water availability, all of which vary regionally.
2	3	4	"Zheng et al. (2017) confirmed the pattern..." should be modified to say "Zhen et al. (2017) confirmed the pattern of observed greening..." As written, and in the context of the previous paragraph, the current text suggests that Zheng et al. 2017 confirmed the pattern of greening being related mainly to increased CO2, a factor which was not examined in their study. Changed: confirmed the pattern of greening, noting that over thirty years it had added 8 percent...
2	3	4	Without the addition of the appropriate context in the preceeding paragraph (my notes 3, 4, and 5) the remainder of this paragraph could be misinterpreted to mean that the impacts of greening on air temperature are attributable back to a positive effect of CO2 on greening. Rewording has addressed this.

2	3	4	The sentence "Greening has been observed globally but Chen et al. (2019) noted that China and India were exploiting it through land management changes" is misleading. Chen et al. (2019) shows that China and India are <i>causing</i> the observed greening in large areas by intentionally increasing the land area occupied by forests (China) and increases in harvested area in croplands, mainly due to multi-cropping, heavy fertilizer use, and expanded irrigation (China and India). It would be more accurate to say "Greening has been observed globally, but Chen et al. (2019) represent a disproportionate fraction of the total greening trend, accomplished through intentional increases in forest area (China), and expansion of multi-cropping, fertilizer use, and irrigation in croplands (China and India)." Changed: "Greening has been observed globally. Chen et al. (2019) show that in China and India much of it is driven by land management changes"
2	3	3 and 4	Given the changes suggested in these two paragraphs, it raises the question of whether the section title (CO2 and global greening) is itself misleading. That title wrongly suggests that all the content to follow is reflective of a direct influence of CO2 on the observed global greening trends, when the literature presents a much more <i>nuanced</i> interpretation. The changed to CO2 as a contributor to global greening
2	3	3 and 4	It would be worth noting in this section that the multiple remote sensing-based datasets for LAI changes have remarkable differences, especially when comparing the figure 2.1 reproduced from Zhu et al. 2016 with the MODIS-based data record from Chen et al. 2019 (their figure 1). That means it is difficult to place a lot of confidence in the attribution of changes in different regions to different factors. See my powerpoint slide 2 for a comparison of the four datasets used in Zhu et al. 2016 and Chen et al. 2019. Thank you for this information. It is an interesting point but gets into more specialized detail than is necessary for this report.
2	3	5	The authorship for the Haverd et al. 2020 citation is not correct in the References section for this chapter. The correct citation is: Haverd, V., B. Smith, J. G. Canadell, M. Cuntz, S. Mikaloff-Fletcher, G. Farquhar, W. Woodgate, P. R. Briggs and C. M. Trudinger 2020. Higher than expected CO2 fertilization inferred from leaf to global observations. Global Change Biology 26(4): 2390-2402 DOI: https://doi.org/10.1111/gcb.14950. Corrected
2	3	5	A more recent and more comprehensive study (Keenan et al. 2023) reexamined multiple data constraints and concluded that the model used in Haverd et al. 2020 (CABLE) is outside the likely range of CO2 fertilization effects, with higher CO2 fertilization than indicated by multiple independent constraints. At the very least this more recent paper should be cited in the report, with text indicating that the central estimate from the TRENDY models is more likely to be in the range of observational constraints than is the CABLE result. As it stands the report suggests that the latest science indicates that most models have underestimated the fertilization effect, and the literature doesn't currently support that conclusion. See: Keenan, T. F., X. Luo, B. D. Stocker, M. G. De Kauwe, B. E. Medlyn, I. C. Prentice, N. G. Smith, C. Terrer, H. Wang, Y. Zhang and S. Zhou 2023. A constraint on historic growth in global photosynthesis due to rising CO2. Nature Climate Change 13(12): 1376-1381 DOI: 10.1038/s41558-023-01867-2. Added sentence: However Keenan et al (2023) estimated a lower fertilization rate more in line with models.

2	4	1	The attribution analysis in the Chen et al. 2024 paper is very sensitive to the choice of multiple regression vs. partial correlation (their figures 3 vs S7, respectively), and in both cases they seem to misattribute the significant increases in LAI in India and China to CO2 rather than to changes in landcover and cropping practices. This kind of regression-based attribution is less informative than the model-based inference used for example by Zhu et al. 2016. The description of sensor calibration issues in this paper is also very cursory. It would probably be better to refer to the review paper of Piao et al. 2020 to support the claim of continued greening. Piao, S., X. Wang, T. Park, C. Chen, X. Lian, Y. He, J. W. Bjerke, A. Chen, P. Ciais, H. Tømmervik, R. R. Nemani and R. B. Myneni 2020. Characteristics, drivers and feedbacks of global greening. Nature Reviews Earth & Environment 1(1): 14-27 DOI: 10.1038/s43017-019-0001-x. Thank you for the comment. We have taken out the details about the Chen et al. analysis and say simply “Piao et al. (2020) and Chen et al. (2024) report that the greening trend continues with no evidence of slowdown, and CO2 fertilization remains the dominant driver”
2	4	2	The section 2.1.2 (Photosynthesis and CO2 levels) lacks coherence. It seems to be a collection of uncontroversial points about the ability of increased CO2 concentration to stimulate plant growth and reduce water loss through stomates. Figure 2.2 seems intended to show that the current levels of CO2 are not high compared to levels experienced in Earth's deep geologic past. A balanced treatment of the topic might call out the rate of change in CO2 concentration from human-caused emissions, and how both that rate and the absolute levels diverge radically from rates of change and ranges of variability over the past hundreds, thousands, and hundreds of thousands of years. See my powerpoint slide 3 for an example set of figures that might be included. In response to another reviewer, we have added the sentence “While the modern rate of change in CO2 is likely high compared to all prior intervals, the geological evidence is that plants and animals evolved under much higher CO2 levels than at present.”
2	7	4	The report suggests that the topics of CO2 fertilization and increased water use efficiency are not adequately treated in recent IPCC reports. In IPCC's AR6 WG I report there is an entire chapter devoted to an assessment of carbon cycle processes (Chapter 5, Global Carbon and Other Biogeochemical Cycles and Feedbacks). This is precisely where one would expect to find information about CO2 fertilization and water use efficiency. In the Executive Summary for the chapter is the following bolded headline: Increases in atmospheric CO2 lead to increases in land carbon storage through CO2 fertilization of photosynthesis and increased water use efficiency (high confidence). The associated Executive Summary text includes reference to two figures, three chapter sections, and a cross-chapter box all dealing directly with this topic. The Executive Summary text also references the important topic of nutrient dynamics and their interactions with fertilization and ecosystem-climate feedbacks, a critical nuance of the interaction of CO2 and vegetation productivity that is ignored in the report reviewed here. We acknowledge that the topic is addressed in the body of IPCC reports. But we draw attention to its omission from summary documents.

2	8	2	Section 2.2.2 (Coral reef changes) has a strong focus on past examples of early investigations that are later updated. There is also a note about the difficulty of publishing negative results, with a quote that this pattern "is true across all of science". Given the objective of this report, I suggest a shift in tone away from complaints about the kinds of papers that do and don't receive media attention, and instead highlight the latest synthesis of results about the likely future of coral reefs. A relatively recent synthesis paper (Cornwall et al. 2021) in a well-respected journal (PNAS) based on collated data from 233 locations worldwide projects that 37% of reefs will stop accreting and thus fail to keep up with sea level rise by 2050 under the low emissions RCP2.6 scenario, and no reefs would be able to accrete at a rate to stay ahead of sea level rise by 2100 under either RCP4.5 or RCP8.5. This analysis finds that most of the loss of net accretion rate is due to bleaching events, and a minority of the impacts are due to "direct physiological impacts of ocean warming and acidification on calcification or bioerosion". C.E. Cornwall, S. Comeau, N.A. Kornder, C.T. Perry, R. van Hooidek, T.M. DeCarlo, M.S. Pratchett, K.D. Anderson, N. Browne, R. Carpenter, G. Diaz-Pulido, J.P. D'Olivo, S.S. Doo, J. Figueiredo, S.A.V. Fortunato, E. Kennedy, C.A. Lantz, M.T. McCulloch, M. González-Rivero, [...] & R.J. Lowe, Global declines in coral reef calcium carbonate production under ocean acidification and warming, Proc. Natl. Acad. Sci. U.S.A. 118 (21) e2015265118, https://doi.org/10.1073/pnas.2015265118 (2021). This is a model projection study whereas for the very limited purpose of this section we are focusing on current and historical observations.
3	14	1	The study by Connolly et al. 2021 uses a linear regression approach to estimate the amount of variability in surface temperature trends that could be explained by total solar irradiance time series. Their approach is to first model the temperature trend with TSI as the dependent variable, and then compare the residuals from that fit to anthropogenic forcing factors. This is an obviously biased approach, and in fact a full paper was published the following year (in the same journal) laying out the erroneous methods and the faulty conclusions they produce (Richardson and Benestad 2022). Richardson, M. T. and R. E. Benestad 2022. Erroneous use of Statistics behind Claims of a Major Solar Role in Recent Warming. Research in Astronomy and Astrophysics 22(12): 125008 DOI: 10.1088/1674-4527/ac981c. While I agree with R&B that a partial regression is biased unless the regressors are orthogonal, Connolly et al. replied in 2023 at https://iopscience.iop.org/article/10.1088/1674-4527/acf18e in which they used updated AR6 data and reproduced their main result using multiple regression. However they also emphasized that it is only one of many possible results depending on choice of solar proxy and surface T record,
3	18	1	Section 3.2.2 (The carbon cycle relating emissions and concentrations). There is no new information or analysis presented in the chapter, and it relies heavily on Global Carbon Project information. Figure 3.2.3 is described as showing a seven-fold difference in the recent trend in land carbon sink estimates across land models, and this is given as a potential driver of uncertainty in future carbon cycle and climate change predictions. Variation among models is well known and is one of the reasons exercises like CMIP and GCP rely on multi-model ensembles when assessing the state of the science. The distribution of results looks fairly typical in this diagram, with a strong central value around 1.3 to 1.3. We're not claiming to have added new information, just reviewing what's out there.

12	129	3	The following sentence is misleading: "And even if global emissions were to stop tomorrow, it would take decades or centuries to see a meaningful reduction in the global CO2 concentration and hence human influences on the climate." It is true that bringing global emissions immediately to zero would not reduce CO2 concentrations on short timescales. It is not true that this same scenario means no reduction in human influence on the climate. We must compare the zero emissions scenario to something, and the most meaningful comparison is the current trajectory of increasing emissions and the resulting super-linear increase in concentration. So by reducing emissions to zero we clearly avoid the expected increases in concentration and the expected increases in radiative forcing together with increased near surface air temperature and all the other expected consequences. So our change in action - going to zero emissions - does in fact have an immediate and growing human influence on the trajectory of the climate system. That is not an argument for reducing our emissions to zero tomorrow, it is just an argument for logic and clarity in how we talk about consequences of different actions. The sentence in the text is correct. It's also true to say that going to zero emissions would, over time, open up a widening gap between observed and counterfactual. But realistically we are only looking at marginal changes. The stock-flow mismatch is one reason IAMs have not recommended going to zero emissions.
12	129	5	Regarding the sentence "The practice of referring to unilateral U.S. reductions as "combatting climate change" or "taking action on climate" therefore reflects a profound misunderstanding of the scale of the issue." This makes the dubious assumption that when people refer to "combating climate change" or "taking action on climate" they are ignorant of the scale of the problem. Instead, it is probably more justifiable to assume that it is well-understood by most people who advocate for these local and national changes that the major emitters must act together to make a big difference in global emissions. It is at least worth considering, in the context of this report, that a path toward action at the necessary scale will require local and national action from a broad consortium, and that individual advocacy is a useful and necessary starting point for shifting the inertia of global policies. Sentence modified to read "The practice of referring to unilateral U.S. reductions as "combatting climate change" or "taking action on climate" on the assumption we can stop climate change therefore reflects a profound misunderstanding"

ch	pg	para	Comment or Suggested Edit
Overarching	Overarching	Overarching	• I would like to thank the authors for the opportunity to provide comment on this report. Providing decision-makers with information about the impacts of carbon dioxide emissions on the U.S. climate is critical as they face consequential decisions about the future of energy, agricultural, and environmental policy. Assessments of the science such as IPCC reports and the NCA benefit from undergoing robust review including expert review, public review, and government review with review comments and responses made publicly available. Similarly, important government reports and regulations undergo inter-agency review and are published in the Federal Register for a full notice and comment process and publish a public response to comment. While these processes are far from perfect, they offer the opportunity for improvement and help lend authority to the final products, and I highly encourage you to undertake a similar robust and open peer review process for this report to enhance the report’s credibility in the eyes of decision-makers and the public.
Executive Summary	viii	1	• There is a disconnect between the stated intent of the report by the authors in the preface, “whether carbon dioxide emissions endanger the U.S. public,” and this executive summary, as well as the organization of the report. Would suggest aligning by either reframing the focus in the preface to better match the organization of the report, or better aligning the report organization with the stated focus. Preface has been reworded and the statement eliminated. We have also changed the title to clarify this
Preface	ix	3	• “While the report is intended to be accessible to non-experts, we have omitted some introductory or explanatory material that can easily be accessed elsewhere. Nor have we attempted to survey entire literatures related to the topics covered.” • While this approach is understandable given the time constraints this report was produced under, given the level of visibility this report will have, it potentially opens up the report to criticism of being misleading by omission – focusing on areas where the authors disagree with the broader scientific literature and omitting areas large areas that are known about climate where there is less disagreement. See our responses to the previous comment
Preface	ix	3	• “We have focused as much as possible on literature published since 2020 and referenced previous IPCC and NCA assessment reports. We have also used data through 2024 where possible.” • This is a good approach for this report; however, much of the literature cited is from pre-2020, and many of the references to IPCC and NCA reports are to older reports (e.g. NCA 4 and IPCC AR 4&5) instead of NCA 5 and IPCC reports from the 6th assessment cycle. In these sections that heavily cite this older literature, it is not clear if the newer literature does not address these points or if the issues raised in the older literature are resolved or significantly changed in the newer literature. We have included reference to previous assessment reports where the analysis is insightful and not superseded by later reports. We note that we regard NCA4 as far more credible than NCA5

Preface	ix	3	• The IPCC’s mandate is to assess the scientific literature comprehensively, including a range of views, and report disagreements. This report has a tendency to selectively quote the IPCC focusing on disagreements and excluding areas where scientists agree. This approach risks giving a misleading picture of the state of the science through omission. A possible way to address this is to have a section summarizing areas of agreement by pointing to all statements of fact and high confidence statements from IPCC AR6 before digging into the more controversial areas with disagreement. This could be a short discussion about areas of agreement up front before explaining the focus of this report on these areas of disagreement, with the IPCC statements of fact and high confidence statements included in an appendix. • The report also includes only a very small subset of the literature since 2020. The authors should describe the approach to how they selected the literature included, and if possible, point to any systematic ways excluded literature could lead to different conclusions. • It is also notable that there a fair amount of new analysis in this report done by the authors that has not yet been peer reviewed. It would be good to flag these spots with a note about why these are needed while other related peer reviewed articles are excluded. This is addressed to some extent in the revised Preface
1	2	1	• If the goal of this report is to discuss links between CO2 and climate, this is an odd way to begin the report with a focus on differences between CO2 and criteria air pollutants. It clears up a confusion of language that persists
1	2	1	• “It is an issue of concern because of its potential effects on the global climate, a complex issue that occupies much of this report.” Suggest deleting the word potential as the existence of the effect is not disputed. Deleted: “potential”
1	2	4	• “But the primary concern about CO2 is its role as a greenhouse gas (GHG) that alters the earth’s energy balance.” Would add “warming the planet” to the end of this sentence as the qualitative effect is not debated and the concern is not about the energy balance itself but about the warming effects of the imbalance. Added: “warming the planet”
2	3	1	• “The recent decline in pH is within the range of natural variability on millennial time scales. Most ocean life evolved when the oceans were mildly acidic. Decreasing pH might adversely affect corals, although the Australian Great Barrier Reef has shown considerable growth in recent years.” • Note that the IPCC Special Report on the Ocean and Cryosphere in a Changing Climate (SROCC) finds that pH decline has very likely emerged from natural variability. SROCC SPM A.2.5, “Open ocean surface pH has declined by a very likely range of 0.017–0.027 pH units per decade since the late 1980s, with the decline in surface ocean pH very likely to have already emerged from background natural variability for more than 95% of the ocean surface area.” We have left this out since it relates to attribution, which is beyond the scope of what we are addressing in this chapter
3	15	3	• “Instead, the IPCC has created various sets of scenarios meant to span a plausible range of possibilities” • Suggest changing “created” to “assessed” as the scenarios are created by the science community not the IPCC. We have changed the wording to “used”
3	16	4	• “The IEA trends, which are based on plausible extrapolations of current economic and demographic trends, run below the entire envelope of RCP projections, implying not only that RCP8.5 is dubious, but that even lower emission scenarios are unlikely.” • Burgess compares IEA to the two highest SSP-RCP scenarios, not to the full range. The last sentence of the paragraph is incorrect – the range in Burgess is only no climate policies which doesn’t include the lower RCPs. The Burgess diagram is poorly-labeled and hard to understand. The text has been revised added a reference to Pielke Jr et al 2022 which has a better presentation.
3	17	2	• “As of 2023, global CO2 emissions have been tracking well below SSP7.0 and are even below SSP2-4.5.” • Note that near-term emissions are not necessarily a good predictor of long-term emissions, as the latter will depend on changes in population, demographics, technology, and policy that do not have the same influence on near-term emissions. Noted

3	22	6	• “In summary, while there is clearly warming in the land record, there is also evidence that it is biased upward by patterns of urbanization and that these biases have not been completely removed by the data processing algorithms used to produce climate data sets.” • Quantitative info would be helpful here. How large is the warming? How large might the bias be? This is a topic that needs revisiting. The quantitative estimates are 15+ years old
4	26	1	• The Chapter Summary doesn’t really capture the full tax of the chapter. Would suggest: Defining ECS/TCS, reporting values for ECS and uncertainties/methods, then reporting values for TCS, preferably with a sentence on what those values imply The terms are defined in the chapter
4	26	2	• “This raising of the lower bound is less justified; evidence since the AR6 finds the lower bound of the likely range to be around 1.8°C.” • This isn’t really discussed comprehensively in the chapter. It is discussed, in context of Nic Lewis’ research
4	26	3	• “The climate’s response to increasing concentrations of CO2 is central to the scientific debate on anthropogenic climate change” • Suggest rewording this to, “The magnitude of the climate’s response...” As currently worded it could be a bit misleading since the qualitative effect (i.e. increasing CO2 concentrations lead to increases in global average temperature) is not disputed – the uncertainty is around the magnitude of the effect. Reworded
4	26	6	• The text sites Dayaratna (2016), but this should be a reference to Dayaratna (2017) as listed in the reference section. Fixed
4	26	6	• “If ECS is very high (above 4.5°C) immediate aggressive emission controls become more imperative, whereas no CO2 emission controls are economically justifiable for ECS below 2.0°C (Dayaratna et al. 2016, 2020).” • This could use additional clarification as there are low and now carbon technologies that can be economically justified based on cost of energy alone, depending on technology, location, etc. We have left as is. We are not aware of low or new carbon technologies that are affordable and scalable. Likewise. If energy conservation pays for the low carbon tech people would have been adopting them already.

4	26	6	• “Obtaining a precise estimate is impossible, so policy making needs to account for the uncertainty.” • The Social Cost of CO2 (SC-CO2) as used by the USG in regulatory impact analyses has always accounted for uncertainties in the ECS. EPA (2016) used a probability distribution for the ECS calibrated to IPCC AR4 findings (the most recent values available at the time) to harmonize the approach for the three models used (FUND, DICE, and PAGE). EPA (2023) updated the SC-CO2 in response to NAS recommendations (NAS 2017) to include a much fuller representation of the uncertainties in both ECS and TCR, and utilized a modular approach incorporating the Finite amplitude Impulse Response (FaIR) climate model, a reduced complexity climate model, with ECS and TCR distributions calibrated to IPCC AR6 to represent this uncertainty. • The Dayaratna (2017 & 2020) papers referenced here analyze sensitivity of a subset of the older models used in EPA (2016) to just ECS estimates. It would be helpful to address the advances in the broader literature that are discussed and represented in EPA (2023) as the focus of this report is on literature published since 2020. • EPA (2016) “Technical Support Document: Technical Update of the Social Cost of Carbon for Regulatory Impact Analysis Under Executive Order 12866 Interagency Working Group on Social Cost of Greenhouse Gases, United States Government.” https://www.epa.gov/sites/default/files/2016-12/documents/sc_co2_tsd_august_2016.pdf • EPA (2023) “Supplementary Material for the Regulatory Impact Analysis for the Final Rulemaking, ‘Standards of Performance for New, Reconstructed, and Modified Sources and Emissions Guidelines for Existing Sources: Oil and Natural Gas Sector Climate Review’ – EPA Report on the Social Cost of Greenhouse Gases: Estimates Incorporating Recent Scientific Advances.” Docket ID No. EPA-HQ-OAR-2021-0317. https://www.epa.gov/system/files/documents/2023-12/epa_scghg_2023_report_final.pdf • NAS (2017) “Valuing Climate Damages. Updating Estimation of the Social Cost of Carbon Dioxide” https://nap.nationalacademies.org/catalog/24651/valuing-climate-damages-updating-estimation-of-the-social-cost-of Most of the topics are touched upon in Ch 11. The newer EPA approach (e.g. the FaIR model) don’t reflect a fundamentally different approach than before. Will discuss below.
4	28	2	• “Because of concerns about model tuning and the high sensitivity to cloud parameterizations, the IPCC AR6 (2021) did not rely on climate model simulations in their assessment of climate sensitivity, relying instead on data-driven methods.” • AR6 WGI 7.5.5 states this a little differently. IPCC’s assessment wasn’t “directly informed” but “ESMs remain essential tools.” Our statement is ok as is
4	29	3	• “For AR6, the IPCC placed primary weight on the results of Sherwood et al. (2020)...” • The IPCC assessed a large number of studies, not just Sherwood (2020). Our statement is ok as is. Sherwood et al was predominantly relied upon
5	32	1	• It would be helpful for the reader for this chapter to start with a summary figure (e.g. like IPCC AR6 WG1 – SPM.1 or Fig. 3.4). Currently, the chapter jumps right into details that will be difficult for anyone who is not a climate scientist or already versed in these issues to understand or interpret. Too late in the process to add a new figure
5	33	Figure	• “Figure 5.1 CMIP6 Average surface temperature range across 33 models and standard deviation using SSP5-85 scenario.” • SSP5-85 is an excessively high scenario that is not representative of current expectations of emissions projections. Suggest changing the figure for on with data from a less misleading and more representative scenario. The use of 8.5 doesnt make much difference given high spread for historical period, but we agree this should be changed for a subsequent version of the Report

5	34	1	• There have been extensive published comments and responses on the approach used in Scafetta (2023). Can these be reflected here? • G.A. Schmidt, G.S. Jones, and J.J. Kennedy, "Comment on "Advanced Testing of Low, Medium, and High ECS CMIP6 GCM Simulations Versus ERA5-T2m" by N. Scafetta (2022)", Geophysical Research Letters, vol. 50, 2023. http://dx.doi.org/10.1029/2022GL102530 • N. Scafetta, "Reply to "Comment on 'Advanced Testing of Low, Medium, and High ECS CMIP6 GCM Simulations Versus ERA5-T2m' by N. Scafetta (2022)" by Schmidt et al. (2023)", Geophysical Research Letters, vol. 50, 2023. http://dx.doi.org/10.1029/2023GL104960 • Z. Hausfather, K. Marvel, G.A. Schmidt, J.W. Nielsen-Gammon, and M. Zelinka, "Climate simulations: recognize the 'hot model' problem", Nature, vol. 605, pp. 26-29, 2022. http://dx.doi.org/10.1038/d41586-022-01192-2 • S. Jain, A.A. Scaife, T.G. Shepherd, C. Deser, N. Dunstone, G.A. Schmidt, K.E. Trenberth, and T. Turkington, "Importance of internal variability for climate model assessment", npj Climate and Atmospheric Science, vol. 6, 2023. http://dx.doi.org/10.1038/s41612-023-00389-0 The Schmidt et al paper is a comment on a different paper of Scafetta's and focuses on his t-statistic, which isn't pertinent to the paper we cite (from Climate Dynamics). Regarding the Jain et al paper, Scafetta uses ensemble ranges within each ECS type and doesn't simply rule models out but shows that some perform well for TAS. Relevance of the Hausfather et al paper is not clear.
5	34	4	• "Spencer (2024) has also provided a useful summary of the Model-Observation mismatch by comparing trends in surface temperature data products with those in individual climate models, as summarized in Figure 5.3; most climate models show substantially more warming than the observations since 1979." • It is worth noting that IPCC projected temperatures do not use all models and instead focus on those that best reproduce observations. So while some models may show warmer or cooler temperatures than observations, that does not imply that projections from large assessments reflect those biases. Our text speaks to the collection of CMIP6 climate models; some of those with the largest amount of warming are widely used models from the US and UK
5	37	3	• "Hence, we assess with medium confidence that CMIP5 and CMIP6 models continue to overestimate observed warming in the upper tropical troposphere over the 1979–2014 period by at least 0.1°C per decade," • "Notably, despite the accumulation of evidence of excess model warming the IPCC assigns only medium confidence to the existence of a warming bias." • This reflects a more general issue with IPCC confidence statements where the inclusion of a quantification reduces the confidence they include. Here the full statement with the quantification of 0.1C per decade is assigned medium confidence whereas a statement about the existence of the bias without a specific quantification would receive higher confidence. Noted
5	39	Figure 5.7	• Does this update with data through 2024 use the same models that Christy and McNider (2017) used, or does it use updated versions for the models as well? This would be helpful to clarify here and in other places where figures are produced with updated data. Excellent catch. Fig. 5.7 uses CMIP-6 models, this is now stated in the caption as an update of Christy and McNider with CMIP6 output. Change made
5	39	3	• "The IPCC AR6 did not assess this issue." • Was this assessed in IPCC AR5? Many of the citations here could have been assessed then. Is there any more recent evidence on these issues that should be referenced here? IPCC has never addressed this issue with rigor, but does reluctantly note the discrepancy.
5	40	1	• Are there more recent papers or data available? All the info here is AR6 or pre-AR6, but more recent years have shown more surface warming. It would be interesting to show stratospheric temperatures for these more recent years as well to see how this relationship holds. Santer et al. (2023) has been added.

5	40	5	• Trends in snow cover depend on both changes in precipitation and changes in temperature. It would be helpful if these were discussed separately rather than showing the net effect for one region/season. Context for statement is that the models get it wrong
6	47	1	• This chapter relies solely on observations and does not include other lines of evidence, it would be helpful to discuss what these other lines of evidence indicate to avoid introducing potential biases (e.g. what do process based lines of evidence show?) Yes we rely primarily on observations. We have added some text regarding process-based analyses, notably in the section on hurricanes
6	47	1	• The chapter summary does not fully capture the chapter or the science. Suggest talking about the extremes affected by climate, the extremes not affected by climate, and those where the data isn't sufficient to establish a trend. We think our summary is appropriate
6	47	3	• “Over geologic time the climate system has generated an (essentially) infinite variety of weather patterns and extremes...” • Just because an event occurred before humans does not mean humans are not influencing the frequency or intensity of such events now. Additionally, if something has happened in the geologic record, but humans have never experienced it before, they may not be prepared for such an event. Changes in frequency and intensity are precisely the issues discussed Adaptation to extreme weather is a separate but important issue
6	50	Figure	• The figure heading has “@RyanMaue” in the title. Looks odd to have a social media handle in the figure. Is the source peer reviewed? We have added reference to an older publication by Maue that describes the analysis
6	52	4	• Insufficient data is different than no effects. It would be helpful to be clear in instances like this whether a conclusion is because the data supports it, or because there is insufficient data to say the opposite. This is covered in the last para of the section
6	53	Table	• Could you also show a similar table with hurricane precipitation? Not in this round. Very hard to get historical rainfall going very far back
6	53	5	• The last two paragraphs show findings from NCA4. With the interest in focusing on recent literature, what does NCA5 say about these issues? Does it not cover this issue, does it say the same thing, does it contradict this finding? NCA5 statements are discussed in the body of the text, see pp 57ff.
6	54	1	• The heading of section 6.3.1 is a bit misleading as this is partly driven by warmer winters and fewer cold days. For the CONUS, the heading is accurate as demonstrated with observational analyses
6	58	Figure	• Figure 6.3.5 shows the number of days greater than or equal to 95F in 6-year periods. Why show this in 6-year periods? That seems like an unusual choice. Would this look different with 5, 10, or 15 year periods? The time series is 126 years long, which is evenly divisible by 6. The point is to compress the depiction of interannual (i.e. natural) variability and examine slower changes. In our decimal system, 5 or 10 years is more commonly used, but with 126 years of data, we did not want to leave an “orphan” column (one with fewer years) compared with the entire time series. This is explained xplained in the Figure caption
6	58	4	• “We measure “Heatwave Days” here as the count of all days in May-Sep each year that exceed the 90th percentile for that day and that lie within a period of at least six consecutive days.” • Why six consecutive days? Would this look different for 5 days, a week, 10 days? Six days seems like an unusual period to pick. As noted in the text we use a metric defined in NCA4 for heat wave days. Regardless, whatever parametric choice is made (i.e. 5 days, 10 days, 85th percentile, etc.) the 1920s to early 1950s dominate the occurrence of heat wave days.

			Added: The pattern of results show below does not depend on the choice of reference period.)”
6	58	4	• The dependence on and subsequent choice of reference periods is worrying. Can multiple reference periods be shown to show the dependence? There will be no change in the fundamental result no matter which reference period is chosen. If the entire period-of-record is used to calculate percentiles, there will be fewer percentile exceedences than if a cold reference period is chosen (as was done in NCA4). However, the shape of the outcome will be the same.
6	59	3	• “The NCA5 directs readers to the website https://www.globalchange.gov/indicators/heat-waves (USGCRP 2023)” • Broken link. The link was live when the report was written, but the Reviewer is correct that it is now broken. Fortunately, we included the figure from that link to demonstrate the unusual start date (1961) and the fact that the sample contained only large cities.
6	61	3 to 6	• This reads as a very selective analysis. With a very tight time constraint and the fact our analysis required the construction of serially complete datasets, we selected regions that were called out by NCA5 and/or the IPCC AR6 as unusual. In future versions we shall have more regions to examine (e.g. upper Midwest, Central Plains, Texas Hill Country).
6	67	2	• Is this section suggesting that climate change is causing the decrease in tornadoes? No, this section doesn’t engage in attribution
6	68	7	• This is another place where it would be better to reference newer literature and NCA5 instead of relying on NCA4. What has changed since then? NCA5 mentions drought many times, but in anecdotal ways without metrics for testing. For example, NCA5 pg 1-23 states. <i>“Between 1980 and 2022, drought and related heatwaves caused approximately \$328 Billion in damages (in 2022 dollars). Recent droughts have strained surface water and groundwater supplies, reduced agricultural productivity, and lowered water levels in major reservoirs, threatening hydropower generation.”</i> This statement simply says that the demand for water was not met by the human-built systems which manage water supplies – there is no metric here that measures a change in the natural water supply. The statements on which we focus relate to whether the supply has been changing – a metric related to the climate system.
6	69	1	• Figure 6.7.1, is this the same indicator used in AR6? We have clarified the text to refer to meteorological drought. AR6 speaks mostly of “Agricultural and Ecological Droughts” which are defined in for various regions as <i>“Agricultural and ecological droughts are assessed based on observed and simulated changes in total column soil moisture, complemented by evidence on changes in surface soil moisture, water balance (precipitation minus evapotranspiration) and indices driven by precipitation and atmospheric evaporative demand”</i>., so we are unsure what specific metric is referenced.” Elsewhere it simply uses the term “drought” or “Meteorological and Hydrological Drought.” AR6 pg 1075 “A drought is a period of abnormally dry weather that persists for long enough to cause a serious hydrological imbalance (Glossary; Wilhite and Glantz, 1985; Wilhite, 2000; Cook et al., 2018). Most droughts begin as persistent precipitation deficits (‘meteorological drought’) that propagate over time into deficits in soil moisture, streamflow, and water storage (Figure 8.6), leading to a reduction in water supply (‘hydrological drought’). Increased atmospheric evaporative demand increases plant water stress, leading to ‘agricultural and ecological drought’.

			The best we can say is the AR6 used a different metric than shown in Fig. 6.7.1 that likely varied from region to region. However, Fig. 6.7.1 has high correspondence with drought-related impacts.
6	69	3	• “In summary there is no evidence of increasing drought frequency or intensity in the U.S. or globally over recent decades.” • This statement is not supported by the citations presented here. AR6 finds that there are trends in agricultural and ecological droughts and this section does not contradict that finding, so this general statement about there being no evidence of increasing drought frequency or intensity is misleading. • AR6 SPM A.3.2 – “Human-induced climate change has contributed to increases in agricultural and ecological droughts in some regions due to increased land evapotranspiration (medium confidence).” Footnote, “Agricultural and ecological drought (depending on the affected biome): a period with abnormal soil moisture deficit, which results from combined shortage of precipitation and excess evapotranspiration, and during the growing season impinges on crop production or ecosystem function in general (see Annex VII: Glossary). Observed changes in meteorological droughts (precipitation deficits) and hydrological droughts (streamflow deficits) are distinct from those in agricultural and ecological droughts and are addressed in the underlying AR6 material (Chapter 11).” We have added “meteorological” to last sentence
6	69	4	• “The IPCC has not provided an assessment of wildfires.” • AR6 WGII SPM discusses wildfire, and WG1 SPM discusses fire weather. • AR6 WGII SPM – B.1.1, B.1.4, Fig. SPM.2, Fig. SPM.3, B.5.2, B.6.2, C.2.3, C.4.2, D.4.2, • AR6 WGI SPM – C.2.4 Restated: The IPCC has not provided an attribution analysis of wildfires
7	75	1	• “The largest sea level increases along U.S. coasts are Galveston, New Orleans, and the Chesapeake Bay regions” • How much? Are there quantifications? The figures show this
8	83	3	• “AR5 Working Group II (Cramer et al., 2014) makes the following statement:” • What does AR6 say on this topic? The science on attribution has evolved since AR5. The AR6 did not make an analogous statement, but the challenges haven’t been resolved
8	84	6	• “There are three areas of substantive criticism of the IPCC’s assessment of the causes of the recent warming...” • Are there citations that can be included here or are these criticisms attributed to the authors? The section describes this adequately
8	84	6	• “IPCC’s policy-informing...” • This phrasing is a bit odd. IPCC reports are policy relevant but policy neutral. This phrasing the direct policy in some way. Deleted policy-informing
8	90	3	• “For comparison, the IPCC estimate of forcing from the increase in atmospheric CO2 compared to preindustrial times is approximately 2.2 W/m2 (Figure 3.1.2).” • This is comparing different time periods. Figure 8.2 includes data for 2023 and 2024, IPCC wouldn’t include the most recent years since it was published in 2021. Suggest updating from the IPCC data to Forster et al. (2024) that updates these indicators. • Forster et al. (2024) “Indicators of Global Climate Change 2024: annual update of key indicators of the state of the climate system and human influence” https://doi.org/10.5194/essd-17-2641-2025 Updated
8	92	2	• “It is not easy to justify a new positive low cloud feedback that began emerging in 2015.” • Explain why this is not easy? Added, “since there is no obvious feedback trigger starting in 2015” e

8	93	4	• “Other columns of IPCC Table 12.12 report on whether anthropogenic signals are expected to emerge this century under RCP8.5, the most extreme forcing scenario. We have omitted these columns for several reasons.” • Can this be done for RCPs other than 8.5? The point of these omitted columns in IPCC Table 12.12 was to show that for some extremes we may not see an effect now, but will in the future. As it is presented here it implies future changes won’t matter. Agree 4.5 would have been better, but this is a query that should be directed to the IPCC
9	104	1	• IPCC AR6 WGII has an assessment of agriculture (both in the SPM and in Chapter 5), and the IPCC also has a special report on Climate Change and Land, that are more comprehensive, recent, and cover a larger range of the literature than discussed here. Please update this chapter to reflect these findings. Need a more specific point of challenge. Other reviewers have brought up specific topics that were either addressed by revisions or are already covered. I think this is one area that goes under the heading of critiquing the mainstream, based on peer-reviewed literature.
10	110	4	• Section 10.1 rightly points out that technological advances have substantially reduced losses from extreme weather events. We are better at forecasting, preparing for, and adapting to extreme weather than we were in the past, and these advances all help reduce impacts. However, our ability to do this in the future depends upon preparing for future changes – we can’t simply assume adaptation will continue, we need to provide information on future risks and how those risks are impacted by climate change in order to inform continued technological advances and future adaptation. Noted
10	111	1	• “In May 2025 NOAA announced it has withdrawn the Billion Dollar Disaster product from publication (Pielke Jr., 2025).” • It would be better to cite an official government source from NOAA announcing this withdraw instead of a blog post commenting on it. Agreed, but this is the best information available from a well-published expert
10	111	2	• Losses from extreme weather and climate events are driven by climate risk (the physical events themselves), exposure to those risks (population and infrastructure impacted by these events), and adaptation to those events (technological advances in forecasting, warning systems, and resiliency). Understanding these risks requires disentangling these drivers in order to prepare for the future. Noted. Such a risk assessment is beyond the scope of this report
10	112	1	• There are additional citations that focus on the net effects of heat and cold related mortality that would be worth discussing and citing in this section. • EPA. 2024. Draft Technical Documentation for the Framework for Evaluating Damages and Impacts (FrEDI). U.S. Environmental Protection Agency, EPA 430-R-24-001. • Cromar et al., 2022. Global Health Impacts for Economic Models of Climate Change: A Systematic Review and Meta-Analysis. Ann. Am. Thorac. Soc. 19, 1203–1212. https://doi.org/10.1513/AnnalsATS.202110-1193OC • Hsiang et al., 2017. Estimating economic damage from climate change in the United States. Science 356, 1362–1369. • Mills et al., 2015. Climate change impacts on extreme temperature mortality in select metropolitan areas in the United States. Clim. Change 131, 83–95 The Cromar meta-analysis included only 7 studies for the US all published before 2015, and not all of them examined both hot and cold extremes. We think the citations we have, which includes the EPA, are adequate.
10	113	7	• Section 10.3.2 has good points about the importance of energy affordability and how energy poverty impacts adaptation behaviors. Cong et al. (2022) is another good citation for this section. • Cong, S., Nock, D., Qiu, Y.L. et al. Unveiling hidden energy poverty using the energy equity gap. Nat Commun 13, 2456 (2022). https://doi.org/10.1038/s41467-022-30146-5 Citation added

11	Chapter	Chapter	• Much of this chapter if focused on older literature on SCC and on applications that fall outside of how the USG has actually implemented SCC. This chapter would benefit from a more explicit focus on EPA (2023) as it represents the latest state of the literature on SCC, shows how the USG applies the SCC for regulatory impact analyses, shows how the USG updated the older IAG era SCC as represented in EPA (2016) in response to the recommendations in NAS (2017), shows how uncertainty is incorporated into SCC estimates, and discusses areas of the literature that are on the frontier, highly uncertain, and not ready to be incorporated into the official USG estimates. • As currently written much of this chapter is focused on these highly uncertain areas of the literature that are not incorporated into USG SCC estimates (e.g. tipping points, and climate change impacts on economic growth). EPA (2023) discusses these issues and why they are not ready to be incorporated into the official estimates. • Furthermore, EPA (2023) estimates of the SC-GHG does incorporate climate benefits such as CO2 fertilization and reductions in cold mortality discussed elsewhere in this report, and this should be noted and discussed. • Finally, this section ignores areas of real controversy in the calculation of SC-GHG as it is used by the USG – namely the question of global vs. domestic benefits and the treatment of discounting when valuing benefits to future generations. These controversial issues deserve far more attention in this report instead of spending so much space on issues on the fringe of the literature that are not even included in USG SC-GHG estimates. • EPA (2016) “Technical Support Document: Technical Update of the Social Cost of Carbon for Regulatory Impact Analysis Under Executive Order 12866 Interagency Working Group on Social Cost of Greenhouse Gases, United States Government.” https://www.epa.gov/sites/default/files/2016-12/documents/sc_co2_tsd_august_2016.pdf • EPA (2023) “Supplementary Material for the Regulatory Impact Analysis for the Final Rulemaking, ‘Standards of Performance for New, Reconstructed, and Modified Sources and Emissions Guidelines for Existing Sources: Oil and Natural Gas Sector Climate Review’ – EPA Report on the Social Cost of Greenhouse Gases: Estimates Incorporating Recent Scientific Advances.” Docket ID No. EPA-HQ-OAR-2021-0317. https://www.epa.gov/system/files/documents/2023-12/epa_scghg_2023_report_final.pdf • NAS (2017) “Valuing Climate Damages. Updating Estimation of the Social Cost of Carbon Dioxide” https://nap.nationalacademies.org/catalog/24651/valuing-climate-damages-updating-estimation-of-the-social-cost-of The new discussion by the EPA simply rehashes longstanding issues. Switching to separable models like FaIR isn’t an advance, because everything still comes down to choice of ECS, discount rate, etc. They add some new damage components but as discussed they jumped on the M17 findings prematurely. Ch 11 focuses a lot on non-SCC topics like the relationship between temperature and growth because that’s been the focus of a lot of research. It’s not the case that this work is too new to be incorporated into SCC calculations. Moore and Diaz (cited) for example incorporated the Dell et al result into an SCC model. The problem is that the temperature signal in growth isn’t robust so economists aren’t convinced it belongs in the models. We don’t discuss the global vs local benefits issue mainly because standard SCC models have always assumed we should measure global effects and we are not offering an argument otherwise.
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11	116-120	Section 11.1	• This chapter has a heavy focus on the relation between climate change and economic growth and how this relationship factors into the SCC. There is an ongoing debate in the literature about persistence in climate damages as measured by changes in economic growth over time; however, these persistence effects on economic growth are not considered in the most recent version of the SC-GHG used by the USG – EPA (2023). This chapter generally ignores the existence of EPA (2023), it is mentioned once in passing, and does not inform readers that these controversial areas of current research are not included in government estimates of the SC-GHG. • EPA (2023) page 58 says, “Given the statistical insignificance of the estimated coefficient on the productivity indicator in the published Howard and Sterner meta-analysis, the SC-GHG estimates presented in this report do not rely on Howard and Sterner’s specifications that include productivity effects. This is consistent with the authors’ recommendations in the published paper, to only consider the inclusion of the productivity impact in sensitivity analysis. However, the question of whether the effects of climate change impacts (e.g., temperature, tropical cyclones, and other extreme weather events) on the economy are only temporary or persistent is an active area of research. Over the past decade, a host of empirical studies have found evidence of temperature changes having persistent effects on the economy (e.g., Dell et al. 2012; Burke et al. 2015; Moore and Diaz 2015; Deryugina and Hsiang 2017; Ricke et al. 2018; Burke and Tanutama 2019; Colacito et al. 2019; Henseler and Schumacher 2019; Kahn et al. 2021; Kumar and Khanna 2019; Bastien-Olvera et al. 2022); this is an important finding because even small changes in economic growth rates accumulate into large economic impacts over time.” Some are discussed in Section 11.1.2 but McKittrick’s review is more up to date.
11	116	1	• “Economists have long considered climate a relatively unimportant factor in economic growth, a view echoed by the IPCC itself in AR5.” • It would be good to update this to the relevant conclusions in AR6 to reflect the more recent literature. AR6 didn’t have an economics chapter comparable to AR5.
11	117	5	• This section makes an important point, much of the public does not understand that climate impacts are relative to projected future without climate change and need to be put in the context of economic growth and a future where our decedents are better off than we are today. O’Neill (2023) is a useful citation here, finding that, “Climate change research and assessments, including the most recent IPCC report, paint an increasingly dire picture of the future. However, the assumption that the future will be worse than the present may be wrong for many aspects of human well-being.” • O’Neill, B.C. Envisioning a future with climate change. Nat. Clim. Chang. 13, 874–876 (2023). https://doi.org/10.1038/s41558-023-01784-4 Citation added
11	119	3	• “Overall they could not detect a temperature effect on GDP or GDP growth, and they estimated the 95 percent confidence interval for the impact on global growth as of 2100 even under the exaggerated RCP8.5 warming scenario spans .86 percent to +388 percent. In other words the net effect is likely positive but too uncertain to distinguish from zero.” • This finding from Newell et al. (2021) needs to be contextualized with their complementary finding that models excluding growth effects and relating temperatures to GDP levels are far more certain. This is important to include since the actual USG application of SC-GHG does not include growth effects. • Newell et al. (20201) states, “Models relating temperature to GDP levels yield climate impact estimates that are far more certain. The best such models imply GDP losses by 2100 of 1–3%, consistent with damage functions currently embedded in the major integrated assessment models that underpin the U.S. social cost of carbon.” The discussion in this section is focused on the effect of dT on growth. The discussion of how dT affects the level of income is what 11.1.1 focuses on. It’s very regionally heterogenous so the small global average reported in Newell et al. isn’t very insightful.
11	119	6	• “To summarize, economists consider climate a relatively unimportant factor in economic growth, a view echoed by the IPCC itself in the Fifth Assessment Report.” • Need to update to reflect AR6 findings. See above

11	121		• “The concepts of estimation and uncertainty do not readily apply to SCC calculations.” • The discussion of uncertainty in IAMs should reflect EPA (2023) which incorporated many major advances in the treatment of uncertainty in integrated assessment modeling. See section A.8 of EPA (2023) for a summary of the quantified sources of uncertainty in the SCGHG estimates presented in EPA (2023) • EPA (2023) “Supplementary Material for the Regulatory Impact Analysis for the Final Rulemaking, ‘Standards of Performance for New, Reconstructed, and Modified Sources and Emissions Guidelines for Existing Sources: Oil and Natural Gas Sector Climate Review’ – EPA Report on the Social Cost of Greenhouse Gases: Estimates Incorporating Recent Scientific Advances.” Docket ID No. EPA-HQ-OAR-2021-0317. https://www.epa.gov/system/files/documents/2023-12/epa_scghg_2023_report_final.pdf Our point is that the key underlying parameters are uncertain, and the range of SCC estimates merely reflects those uncertainties. The fact that the EPA proposes ways to compute distributions of the SCC doesn’t affect this point, it follows from it.
11	121	3	• “Economists use IAMs to compute the SCC. Two of the best known are the Climate Framework for Uncertainty, Negotiation and Distribution (“FUND”, Tol 1997) and Nordhaus’ DICE.” • This is old literature. Given the advances since FUND and DICE were the leading SCC models, and this report’s focus on literature post-2020, this section should be updated to focus on the models used to calculate the SC-GHG in EPA (2023) which represents advances over the approach used in FUND and DICE in response to NAS (2017) recommendations. Added a mention of this. • EPA (2023) “Supplementary Material for the Regulatory Impact Analysis for the Final Rulemaking, ‘Standards of Performance for New, Reconstructed, and Modified Sources and Emissions Guidelines for Existing Sources: Oil and Natural Gas Sector Climate Review’ – EPA Report on the Social Cost of Greenhouse Gases: Estimates Incorporating Recent Scientific Advances.” Docket ID No. EPA-HQ-OAR-2021-0317. https://www.epa.gov/system/files/documents/2023-12/epa_scghg_2023_report_final.pdf
11	121	4	• “and a ‘judgmental adjustment’ of 0.5 percent for excluded impacts at 3°C warming” • EPA (2023) is the most up to date representation of the USG approach to SC-GHG incorporating the recommendations of the NAS (2017), and EPA (2023) explicitly does not included an adder for missing damages. This is another example of this chapter focusing on older less relevant literature, and it really should be comprehensively updated to address EPA (2023). Barrage and Nordhaus is a (2024) paper. We don’t endorse their approach, just describe it. But in some respects the EPA approach is already obsolete because the new Administration has abolished the SCC. • EPA (2023), page 59 states, “the model runs performed for this report do not adopt a 25% adder (as used in the DICE model (e.g., Nordhaus 2017)) to account for unknown or missing damages for the meta-analysis-based damage module.” • EPA (2023) “Supplementary Material for the Regulatory Impact Analysis for the Final Rulemaking, ‘Standards of Performance for New, Reconstructed, and Modified Sources and Emissions Guidelines for Existing Sources: Oil and Natural Gas Sector Climate Review’ – EPA Report on the Social Cost of Greenhouse Gases: Estimates Incorporating Recent Scientific Advances.” Docket ID No. EPA-HQ-OAR-2021-0317. • NAS (2017) “Valuing Climate Damages. Updating Estimation of the Social Cost of Carbon Dioxide” https://nap.nationalacademies.org/catalog/24651/valuing-climate-damages-updating-estimation-of-the-social-cost-of

11	122	6	• “Abatement costs: IAMs represent the cost to the economy of reducing CO2 emissions. If CO2 emission reductions are assumed to be inexpensive, then the model will conclude that the optimal policy should aim for deeper emission cuts and vice versa.” • IAMs such as DICE are often used in a mode of calculating the optimal level of abatement where they do indeed compare the SCC with the marginal cost of abatement to find the optimum. However, these abatement costs are not part of the SCC calculation, and as used by the USG for the purpose of regulatory impact analyses, this feature of these IAMs is not used. Instead the USG has other more appropriate tools for analyzing the costs of specific regulations. This distinction is important here as the assumptions about the cost of emissions abatement does not have any bearing on the calculation of the SCC. The paragraph has been rewritten to clarify this.
11	122	7	• “For example, in 2023 the U.S. Environmental Protection Agency raised its preferred SCC value about 5-fold over the estimates it had issued ten years earlier. This is not because new data had been collected or better mathematical methods had been invented, but because new assumptions had been used, and the validity of those assumptions was a separate question.” • This paragraph contains the only reference to EPA (2023). First, the characterization of this update as only because new assumptions are used is factually incorrect. EPA (2023) represents a significant change to how the USG calculated the SC-GHG moving away from the FUND/DICE/PAGE models used in the IWG era (see EPA (2016)) to a modular approach as recommended by NAS (2017). This chapter needs to address the NAS (2017) recommendations and the EPA (2023) response to those recommendations to capture how SC-GHG was actually used by the USG and how the literature has evolved over time. The tabulations in EPA 2023 p. 81 show that if they had not changed the Ag productivity assumption and added a large mortality cost they’d have ended up with SCC numbers very close to the 2013 IWG numbers. Assuming very low discount rates did the rest. Using the FaIR model with the DSCIM and GIVE damage modules wasn’t the key change, it was • EPA (2016) “Technical Support Document: Technical Update of the Social Cost of Carbon for Regulatory Impact Analysis Under Executive Order 12866 Interagency Working Group on Social Cost of Greenhouse Gases, United States Government.” https://www.epa.gov/sites/default/files/2016-12/documents/sc_co2_tsd_august_2016.pdf • EPA (2023) “Supplementary Material for the Regulatory Impact Analysis for the Final Rulemaking, ‘Standards of Performance for New, Reconstructed, and Modified Sources and Emissions Guidelines for Existing Sources: Oil and Natural Gas Sector Climate Review’ – EPA Report on the Social Cost of Greenhouse Gases: Estimates Incorporating Recent Scientific Advances.” Docket ID No. EPA-HQ-OAR-2021-0317. https://www.epa.gov/system/files/documents/2023-12/epa_scghg_2023_report_final.pdf § NAS (2017) “Valuing Climate Damages. Updating Estimation of the Social Cost of Carbon Dioxide” https://nap.nationalacademies.org/catalog/24651/valuing-climate-damages-updating-estimation-of-the-social-cost-of
11	123	2	• “It should also be noted that the SCC is focused on the social costs of CO2 emissions from fossil fuel use. It does not measure the private marginal benefits to consumers and society from the availability of fossil fuels. Public willingness to pay for fuels of all types indicates the value to society of reliable, abundant fossil energy.” • Two points to make about this section. First, the benefits to society generally accrue from the provision of energy services, not from specific fuels.” A distinction without a difference in this context. Second, the private marginal benefit of the availability of fossil fuels or energy services is not part of the SCC because changes in the cost and provision of the energy services should be part of the other side of the cost benefit analysis. The SCC is used to characterize the benefits of GHG emissions abatement, and the changes in the costs and availability of energy services are part of the cost of emissions abatement. Agreed, changed the wording to clarify
11	125	5	• “the uncertainty of SCC estimates doesn’t mean that other regulatory measures are inherently better or more efficient.” • ‘regulatory measures’ is a poor word choice here as it can be confused with the regulation itself instead of the metric used to assess the regulation. Changed measures to instruments

11	123-125	Section 11.1	• Section 11.2.4 has an interesting discussion of tipping points. This is an active area of research in the field, however it needs to be noted that tipping points or tipping elements are not included in the SC-GHG as used by the USG, see EPA (2023). • EPA (2023), page 3 says, “For example, the modeling in this report omits most of the consequences of changes in precipitation, damages from extreme weather events, the potential for nongradual damages from passing critical thresholds (e.g., tipping elements) in natural or socioeconomic systems, and nonclimate mediated effects of GHG emissions other than CO2 fertilization (e.g., ocean acidification due to CO2 emissions, tropospheric ozone formation due to CH4 emissions). Importantly, this update does not yet reflect interaction effects and feedback effects within, and across, natural and human systems. For example, it does not explicitly reflect potential interactions among damage categories, such as those stemming from the interdependencies of energy, water, and land use. These interactions and feedbacks, and others, were highlighted by the National Academies as an important area of future research for longer-term enhancements in the SC-GHG estimation framework.” Not necessary to say this since EPA is on hold from doing SCC calculations
12	129	1	• This chapter summary is misleading as it is describing change from today’s temperature rather than the effect on future temperatures. A change from today’s temperature is the future temperature. Not sure what the objection is.
12	129	2	• “And even if global emissions were to stop tomorrow, it would take decades or centuries to see a meaningful reduction in the global CO2 concentration and hence human influences on the climate.” • If global emissions were to go to zero tomorrow, CO2 concentrations would stabilize, and global temperature would be approximately constant. Net negative CO2 would result in reductions of concentrations and temperatures. In other words, while reducing global CO2 emissions to zero might not lead to near-term reductions in CO2 concentrations from today’s levels, it would stop future increases in CO2 concentrations. Consistent with what we stated.
12	129	3	• This section is focused on unilateral action, but needs to address questions related to collective action and individual actions by multiple parties when the benefits of abatement and the damages from emissions have extensive externalities. • Kotchen (2021) provides a theoretical perspective on the global vs. domestic SCC question and addresses questions related to how an individual country’s decisions are impacted by the expected choices of other countries given that GHG emissions are globally mixed the benefits of GHG abatement are shared widely. • Kotchen (2021). Which Social Cost of Carbon? A Theoretical Perspective. Journal of the Association of Environmental and Resource Economists. https://doi.org/10.1086/697241 An interesting line of thought but we can’t get into game theoretic models here.
12	129	4	• “Since that rate has been averaging about 50 percent of emissions in recent decades, a reduction of global emissions by 50 percent would (at least temporarily) halt the rise in atmospheric CO2.” • This may be addressed in earlier chapters that this reviewer has not read yet, but the standard framing is that stabilizing CO2 concentrations requires balancing emissions by sources and removals by sinks. Removals by sinks includes both natural sequestration and anthropogenic sequestration (e.g. afforestation if it can be done that increases the overall land sink removal, or direct air capture). It could be useful to clarify this here. The discussion is only an approximation for illustrative purposes
12	129	5	• “Lomborg (2016) estimated that full compliance with the Paris Accord would not stop warming, it would only prevent about 0.1C warming and delay hitting the baseline year 2100 temperature levels by about a decade.” • Note that the Lomborg (2016) finding is related to, “the of impact of the total, global emission cuts implied by all the submitted INDCs,” this is different than ‘full compliance with the Paris Accord’ as the INDCs analyzed in the paper are only the first round of Intended Nationally Determined Contributions submitted ahead of COP-21, and the Paris Accord requires additional Nationally Determined Contributions every 5 years. Since Lomborg (2016) only analyzes these INDCs, and parties have submitted two rounds of NDCs since then, suggest changing the wording here to, “... full compliance with initial commitments submitted ahead of the Paris Accord.” Changed.

12	129	Chapter	• [NOTE: this comment is long and could be difficult to read in excel. It is included here once as a full comment, then again broken into multiple parts for ease of reading. This is PART 1] • Section 12.1 is titled, “The scale of the problem,” is framed around contrasting criteria air contaminants with CO2 emissions; however, there really are three separate issues that need to be disentangled: Stock vs. Flow. • The challenge here is that abatement costs borne by people today benefit people in the future – temporal externalities. • CO2 emissions are a stock pollutant, so impacts depend on the cumulative emissions to date, whereas traditional criteria are a flow pollutant and impacts depend on the emissions at that point in time. Emissions of a stock pollutant going to zero will not affect impacts at that point in time, as the stock remains unchanged, but will have large impacts in the future as the stock will cease to increase until emissions resume; whereas emissions of a flow pollutant going to zero will immediately eliminate impacts, but impacts will have no lasting effects on future impacts as they will immediately resume when emissions resume. • The sentence at the end of the second paragraph of this section reads, “And even if global emissions were to stop tomorrow, it would take decades or centuries to see a meaningful reduction in the global CO2 concentration and hence human influences on the climate.” This is conflating the expectations of how stock and flow pollutants work – if global emissions were to stop tomorrow, the fact that there would be no meaningful reduction in global CO2 concentrations is simply the nature of how stock pollutants work, but there would be very large impacts on future temperatures since the stock would cease to increase. Conflating isn’t the right word. We are pointing out the stock-flow issue. • The stock nature of CO2 emissions is a real challenge for justifying CO2 emissions reductions, but it is centered on justifying the costs of abatement being born by people today in order to create a stream of benefits that grow over time and primarily accrue to future generations. This leads to questions important questions about how decision-makers should think about inter-generational discounting, and what costs should or should not be borne today for the benefit of future generations that will potentially be substantially better off. Dealt with in the IAMs section • It’s worth noting that since CH4 emissions are relatively short lived, they behave much more like a flow pollutant than a stock pollutant. If CH4 emissions went to zero, concentrations would fall relatively quickly, and the temperature effects would happen on a much shorter timescale, but if CH4 emissions fully resumed, concentrations would return to previous levels there would be no lasting impact of the previous reductions. This is to point out that the stock vs. flow distinction also happens between different greenhouse gasses and is not purely a distinction between GHG emission and criteria pollutants. The CH4 case is interesting and we haven’t looked at it. Also natural sources and sinks are complicated for CH4
12	129	Chapter	• [NOTE: this comment is long and could be difficult to read in excel. It is included here once as a full comment, then again broken into multiple parts for ease of reading. This is PART 2] • Globally mixed. • The challenge here is that abatement costs borne by people in one location benefit people in other locations – spatial externalities. • The next issue is that GHG emissions are globally mixed, so that emissions in any one location have impacts all over the world. This is a distinct issue from the stock vs. flow issue, as the CH4 example above showed a flow pollutant can also be globally mixed. One can also consider a stock pollutant that is not globally mixed, such as groundwater pollution that depends on the stock of contaminants accumulating in the groundwater not just the flow in any one time period as one would see in surface water pollution. • The globally mixed nature of GHG emissions is a real problem for justifying climate policy as the benefits of any one country’s actions primarily accrue to people in other countries. Conversely, a country benefits from the actions of every other country, not just their own actions. This is a problem of game theory and collective action, as unilateral action may not be justified even if collective action would be. It’s worth noting that this isn’t just a problem of scale – these same problems arise for globally

			mixed CH4 emissions even though as noted above they behave more as a flow pollutant, and would arise for any globally mixed pollutant regardless of the scale of the problem. There is an extensive literature on transboundary pollution that explores these issues. Agree with all this, but discussing these points wouldn't change our overall conclusions
12	129	Chapter	• [NOTE: this comment is long and could be difficult to read in excel. It is included here once as a full comment, then again broken into multiple parts for ease of reading. This is PART 3] Scale. • The challenge here is that solving the problem requires enormous change and potentially enormous costs. However, individual actions need to be considered at the margin. • As discussed in this section and the comments above, stabilizing CO2 concentrations and stopping the increase in global mean temperature requires balancing anthropogenic emissions and removals by sinks. The level global mean temperature stabilizes at would generally depend on the cumulative CO2 emissions up to the point CO2 emissions on net go to zero (the stock of CO2 emissions), and the level of short-lived non-CO2 emissions at that time (the flow of non-CO2 emissions). This would involve an enormous change to the global energy system. The costs of this change would be highly dependent on the timing of reaching net-zero CO2 emissions – in 10 years by 2035 would involve crippling costs to the global economy, in 100 years by 2125 would involve a far more gradual transition and likely impose minimal costs when discounted back to today. • What is missing in this chapter is an acknowledgement that policy needs to be thought of at the margin. Do the marginal benefits of the policy equal or exceed the marginal costs? This is how economists think of these problems and the essence of how the government conducts cost-benefit analysis to inform decision-makers under E.O. 12866. The case study presented in section 12.2 poses the wrong two question. Showing that a policy's emissions reductions are small relative to global emissions, and the resulting impact on global temperatures is also small is tautological. No individual policy will solve climate change, but that does not mean that the individual policy is not justified. The relevant questions for decision makers are: (1) what is the cost of the marginal ton of emissions reductions by this policy? and (2) are those marginal costs smaller than the marginal benefit of a ton of emissions reductions? In practice, question (2) is very difficult to answer, and requires judgements about how to value benefits to Americans vs. benefits to the rest of the world, how to discount benefits that accrue to future generations, and how to consider the impacts U.S. actions have on the likelihood that other countries will take actions that benefit the U.S. These are difficult questions that need to be answered to justify climate policy, and this section would be improved by acknowledging that these are the questions that need to be addressed instead of the awkward argument that the problem is big, and individual policies are small therefore they can't be justified. Ch 12 is only focused on the scale problem. The reviewer is correct that even if the effects are small that doesn't mean they shouldn't be undertaken. Ch 11 is where the logic of cost-benefit analysis including marginal considerations is explored.

12	129	Chap ter	• [NOTE: this comment is long and could be difficult to read in excel. It is included here once as a full comment, then again broken into multiple parts for ease of reading. This is the full comment] • Section 12.1 is titled, “The scale of the problem,” is framed around contrasting criteria air contaminants with CO2 emissions; however, there really are three separate issues that need to be disentangled: § Stock vs. Flow. • The challenge here is that abatement costs borne by people today benefit people in the future – temporal externalities. • CO2 emissions are a stock pollutant, so impacts depend on the cumulative emissions to date, whereas traditional criteria are a flow pollutant and impacts depend on the emissions at that point in time. Emissions of a stock pollutant going to zero will not affect impacts at that point in time, as the stock remains unchanged, but will have large impacts in the future as the stock will cease to increase until emissions resume; whereas emissions of a flow pollutant going to zero will immediately eliminate impacts, but impacts will have no lasting effects on future impacts as they will immediately resume when emissions resume. • The sentence at the end of the second paragraph of this section reads, “And even if global emissions were to stop tomorrow, it would take decades or centuries to see a meaningful reduction in the global CO2 concentration and hence human influences on the climate.” This is conflating the expectations of how stock and flow pollutants work – if global emissions were to stop tomorrow, the fact that there would be no meaningful reduction in global CO2 concentrations is simply the nature of how stock pollutants work, but there would be very large impacts on future temperatures since the stock would cease to increase. • The stock nature of CO2 emissions is a real challenge for justifying CO2 emissions reductions, but it is centered on justifying the costs of abatement being born by people today in order to create a stream of benefits that grow over time and primarily accrue to future generations. This leads to questions important questions about how decision-makers should think about inter-generational discounting, and what costs should or should not be borne today for the benefit of future generations that will potentially be substantially better off. • It’s worth noting that since CH4 emissions are relatively short lived, they behave much more like a flow pollutant than a stock pollutant. If CH4 emissions went to zero, concentrations would fall relatively quickly, and the temperature effects would happen on a much shorter timescale, but if CH4 emissions fully resumed, concentrations would return to previous levels there would be no lasting impact of the previous reductions. This is to point out that the stock vs. flow distinction also happens between different greenhouse gasses and is not purely a distinction between GHG emission and criteria pollutants. § Globally mixed. • The challenge here is that abatement costs borne by people in one location benefit people in other locations – spatial externalities. • The next issue is that GHG emissions are globally mixed, so that emissions in any one location have impacts all over the world. This is a distinct issue from the stock vs. flow issue, as the CH4 example above showed a flow pollutant can also be globally mixed. One can also consider a stock pollutant that is not globally mixed, such as groundwater pollution that depends on the stock of contaminants accumulating in the groundwater not just the flow in any one time period as one would see in surface water pollution. • The globally mixed nature of GHG emissions is a real problem for justifying climate policy as the benefits of any one country’s actions primarily accrue to people in other countries. Conversely, a country benefits from the actions of every other country, not just their own actions. This is a problem of game theory and collective action, as unilateral action may not be justified even if collective action would be. It’s worth noting that this isn’t just a problem of scale – these same problems arise for globally mixed CH4 emissions even though as noted above they behave more as a flow pollutant, and would arise for any globally mixed pollutant regardless of the scale of the problem. There is an extensive literature on transboundary pollution that explores these issues. § Scale. • The challenge here is that solving the problem requires enormous change and potentially enormous costs. However, individual actions need to be considered at the margin. • As discussed in this section and the comments above, stabilizing CO2 concentrations and stopping the increase in global mean temperature requires balancing
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		anthropogenic emissions and removals by sinks. The level global mean temperature stabilizes at would generally depend on the cumulative CO2 emissions up to the point CO2 emissions on net go to zero (the stock of CO2 emissions), and the level of short-lived non-CO2 emissions at that time (the flow of non-CO2 emissions). This would involve an enormous change to the global energy system. The costs of this change would be highly dependent on the timing of reaching net-zero CO2 emissions – in 10 years by 2035 would involve crippling costs to the global economy, in 100 years by 2125 would involve a far more gradual transition and likely impose minimal costs when discounted back to today. • What is missing in this chapter is an acknowledgement that policy needs to be thought of at the margin. Do the marginal benefits of the policy equal or exceed the marginal costs? This is how economists think of these problems and the essence of how the government conducts cost-benefit analysis to inform decision-makers under E.O. 12866. The case study presented in section 12.2 poses the wrong two question. Showing that a policy’s emissions reductions are small relative to global emissions, and <i>the resulting impact on global temperatures is also small is tautological. No individual policy will solve climate change, but that does not mean that the individual policy is not justified.</i> The relevant questions for decision makers are: (1) what is the cost of the marginal ton of emissions reductions by this policy? and (2) are those marginal costs smaller than the marginal benefit of a ton of emissions reductions? In practice, question (2) is very difficult to answer, and requires judgements about how to value benefits to Americans vs. benefits to the rest of the world, how to discount benefits that accrue to future generations, and how to consider the impacts U.S. actions have on the likelihood that other countries will take actions that benefit the U.S. These are difficult questions that need to be answered to justify climate policy, and this section would be improved by acknowledging that these are the questions that need to be addressed instead of the awkward argument that the problem is big, and individual policies are small therefore they can’t be justified.
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Ch	page	Para	Comment or Suggested Edit
2	5	Fig 2.2	The graph is not relevant to the discussion. It should only, at most, cover the time period since the last glacial maximum, which occurred ~20 kyr BCE, and common practice is to concentrate on the last 1000 years. Figure 2.2 has been deleted
3	11,12	Sec 3.1.1	There is no quantification of the effects of TSI and volcanic aerosols that would lead to modifications of the radiative forcing shown in the figures In Chapter 6 we discuss ranges of TSI forcing that even the IPCC acknowledges.
3	21,22	Sec 3.3	Many of the arguments and references are dated. Further, the challenge posed is to prove a negative, i.e. that the record used by AR6 is not biased by urban heat island effects. On the contrary, while verbally stating uncertainty, the authors cannot make a claim that the AR6 statement is false. Even if one were to accept the Soon, et al value of 0.89 deg C/century, the focus on the land surface warming completely neglects the contribution of the oceans to global surface temperature change, which must be considered in any discussion of surface temperature change. The underlying studies frame statistical tests appropriately to test for the presence or absence of an effect. Yes ,the literature is dated and the topic needs renewed attention. The question is bias in the land record, but no one is arguing all surface warming is due to urbanization.
4	30	Sec 4.4	The statement "TCR is more generally related to peak warming and better constrained by historical warming, than ECS." requires further context. While true that the "peak warming" rate achieves a maximum around 70 years, temperature increases continue for several centuries and approaches the ECS value asymptotically. Further, the Lewis (2023) arguments were placed in context by Sherwood and Forest (2024), that identified multiple recent ECS studies that showed, collectively, that the PDF in their original Sherwood, et al paper could not be narrowed. Additionally, the reference to Sherwood et al (2020) is incorrect. It omits many authors and the authors that are listed are not in the correct order. Reference to peak warming deleted.
5	37, 38	Sec 5.4	The analysis presented of the IPCC AR5 figure, Fig 5.6 in the current document, is outdated and should be removed. The issue IS addressed in the AR6, but not graphically, in the text reproduced by the authors. If the section is retained, it should be moved ahead of Section 5.3 to present the material in the correct order chronologically. The analysis in Fig. 5.6 matches the updated data in Fig 5.7, and it is relevant to show that the information was included in the AR5 but only in a figure in the Supplement. The order of presentation in the chapter is appropriate.
6	57	para 1	The conclusion "The rise in Tmin has been strongly related to the growing presence of manufactured surfaces around the weather stations over the last 100+ years (the so-called urban heat island effect; Section 3.3 and Spencer et al. 2025)." is only found in an author's self-citation of Spencer, et al, (2025) that does not appear in the references at the end of Chapter 6. Further, the trend of greater increases in Tmin relative to Tmax is documented and explained in Easterling, et al (1997). https://doi.org/10.1126/science.277.5324.364 ; Thorne, et al (2016) https://doi.org/10.1002/2015JD024584 . The trend cannot be attributed to UHI effects. We added citations to the literature to substantiate the point.
8	85	para 5	"... datasets with high TSI variability can explain more than 70 percent of the temperature variability since preindustrial times." Variability on what time scales? 11-year cycles? How would this impact the well documented surface temperature increase of > 1 deg C global temperature change since 1900?

			There is a whole spectrum of variability in TSI beyond the 11-year cycle. These are discussed in the papers we reference.
8	85,86,87	"Natural variability of large-scale ocean circulations"	The discussion neglects the influence of vertical mixing on surface temperature. The global ocean has warmed, as documented by increases in global ocean heat content, e.g. Murphy, et al (2009) doi:10.1029/2009JD012105 We do not understand the comment.
8	88,89	Optimal fingerprinting discussion	This section is hardly a balanced and objective review. The report authors are known opponents of optimal fingerprint approaches and have been a loud, but very small minority voice. It is interesting that even they conclude "...these analyses do not falsify results of the IPCC optimal fingerprinting method..." The point is that falsification would require redoing all the underlying studies. The findings however show that the method is flawed and the justification for relying on it is invalid.
8	90,91,92	Section 8.4	This discussion is incomplete and fails to take into account the extensive literature on cloud and surface albedo feedbacks.. There is an extensive discussion about the post 2015 period and recent record annual global surface temperatures, implying, but not stating, that recent changes are the result of natural variability, or decreases in atmospheric aerosol concentrations. Comparing the radiative forcing from these changes with long-term GHG forcing, particularly CO2 is misleading to the average reader. If the reduction in albedo can be tied to atmospheric cloud and aerosol changes, as the authors state, they cannot ignore the likelihood that the changes are the result of anthropogenic factors, i.e. sulfate aerosol reduction through cleaner technology and the well-documented positive low-cloud feedback. The discussion is incomplete because the underlying science is incomplete, as we emphasize. We are surveying a complex area of ongoing research without taking a strong position ourselves.

Full document			In the limited time allowed, this reviewer concentrated on sections related to the physical climate and climate change. Frankly, the document was discouraging. Secretary Wright wants a " ... a more thoughtful and science-based conversation about climate change and energy." I find that this report distorts the science in much the same way as the media coverage about which he complains. Most notable are the omissions or under-reported significant climate changes, such as sea-ice decline, changes in the global water cycle and increases in heat-stress due to increases in absolute humidity (from water-vapor feedback). Further, the examples used by the report authors are narrow and appear to be selected to emphasize uncertainty, while the more rigorously evaluated science that contradicts their limited discussion is ignored, perhaps because it does not fit their narrative. This is unfortunate, because I believe it detracts from the much needed discussion about the future, including energy policies, technologies and trade-offs. I encourage DOE to start the discussion by accepting the well-reviewed and broadly accepted scientific conclusions of the IPCC Working Group 1, with respect to the observations and analyses of climate change in the 20th and 21st centuries. Skepticism regarding predictions/projections of future climate based on posited scenarios is justified. As a member of the physical climate research community, I believe there are better uses for the models (and the computer resources they consume) than scenario projection simulations, to inform the discussion that is urgently needed about the future. Comments noted.
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Chapter Number	pg	para	Comment or Suggested Edit
Foreword	vii	4	The stated charge to the team was to summarize the current state of climate science, but in the Preface the team states that it chose to focus on topics that are downplayed in or absent from current assessment reports. To accurately describe the contents of the report, the Foreword should also state that the report is a summary of the things that the author team thinks have been downplayed in major climate assessment reports. Noted, the Preface has been modified. Our charge was not to summarize the whole of climate science.
Foreword	vii	8	The statement about climate change not being the greatest threat facing humanity is a value judgement. Since the Secretary criticizes others for blurring the line between scientific conclusions and policy recommendations in the second, fourth, and sixth paragraphs of this Foreword, it is hypocritical to offer a value judgement without clearly labeling it as such and noting that such conclusions must necessarily involve one's beliefs in addition to scientific information. We did not write the Secretary's foreword.
Exec Summ	viii	2	It is misleading (cherry picking) to only mention "global greening" (i.e., the influence of elevated CO2 on plant growth) here without also noting the impacts of elevated temperatures and changing precipitation patterns on ecosystem health and agricultural productivity. (This comment is also true of the underlying content on pages 3-7) This issue has been addressed with wording changes to acknowledge the other contributors.
Exec Summ	viii	2	It is misleading to state that ocean pH levels are within the range of natural variability. This statement is not true on the timescales governing the impacts of ocean acidification on ocean ecosystems and fisheries. (This comment is also true of the underlying content on pages 8-9) The statement has been removed.
Exec Summ	viii	3	It is inaccurate to say that "scenarios of CO2 emissions have tended to overestimate observed levels". As the authors themselves note on page 15, it is impossible to reliably predict societal behavior regarding energy production and other sources of emissions, so the climate science community has never provided or suggested that there is a single predictive scenario against which future emissions could be judged. Rather, there has always been a range of projected emissions scenarios, and emissions have always tracked within the envelope of those scenarios. The authors make a reasonable point in the full text about the large number of climate studies that have used RCP 8.5, whereas actual emissions have recently been closer to RCP 4.5. This would be a more appropriate finding to include here in the executive summary. The point being made here is that observations have tracked the low end of the envelopes, a problem going back decades. See Figure 3.2.1
Exec Summ	viii	4	Stating that climate models "offer little guidance on how the climate responds to elevated CO2" is at best an unjustified (and at worst a biased) judgement given that the authors chose to focus only on the shortcoming and deficiencies of these models, and do not account for the progress made in improving the ability of these models to reproduce many key aspects of observed climate change. The same is true for all of Chapter 5. It is informative for readers to see key ways in which models fail to match observations especially those most relevant for policy making. The continuing wideness of the range of responses is an important limitation to their fitness for purpose.

Exec Summ	viii	5	Stating that climate models "generally run hot" is an inaccurate and misleading summary statement. A large number of papers and scientific assessments have validated the ability of climate models to accurately simulate many major aspects of global climate. Like all models, climate models are imperfect, and some models have recently shown a tendency to produce higher climate sensitivities--the IPCC AR6 acknowledged this "hot model problem" and there has been considerable discussion in the scientific literature about it and its implications for impacts studies. But this is a relatively recent phenomenon, and there could be other reasons for the recent mismatch (i.e., under-estimates of the reduction in aerosol cooling due to air pollution controls). A balanced scientific assessment would also note, for example, that climate model projections from the 1970s and 80s have accurately predicting the observed trend in global temperature over the past few decades. Our statements are substantiated by the Figures in Ch 5 (especially Figs 5.2 - 5.4). As we discuss, the problem of the tropospheric mismatch goes back decades.
Exec Summ	viii	6	It is misleading and inaccurate to say that most extreme weather events do not show long terms trends. It is true that some kinds of extreme events, like tornadoes, have not shown appreciable or detectable changes, and that changes in wildfire must account for changes in land management. But a "summary of the current state ofthe science" should acknowledge that there is clear evidence that heat waves are becoming more frequent and more extreme, extreme precipitation and nuisance flooding are becoming more common, etc. (This comment is also true of the underlying content in Part II) The chapter presents a great deal of data that substantiates our summary statement. We also provide clear indications on heatwave trends including where they are increasing in frequency. Our assessments match those in the IPCC and NCA documents.
Exec Summ	viii	6	It is misleading to state that the aggregate sea-level rise trend in U.S. tide gauges is not accelerating, because as noted in the same sentence, there are significant regional differences in SLR (primarily due to differences in land subsidence), so only looking at domestic gague data is insufficient to determine if sea-level rise--an inherently global process--is accelerating, with regional factors superimposed on that global trend. There is clear evidence that global sea level rise is accelerating. Moreover, while land subsidence has offset sea level rise in some U.S. regions, the opposite is true in others, and in those reasons the combined effects of sea level rise and subsidence are posing considerable risks to people and infrastructure in those regions...but this important context is omitted. (The same is true of the sea level rise analysis on pages 75-78.) We state that global sea level rise has accelerated in recent decades. The reviewer needs to provide evidence for the claim of acceleration. SLR is a regionally-heterogeneous phenomenon and we discuss the differences by location. We present tide-gauge data for many locations so readers can see the situation for themselves.
Exec Summ	viii	9	Most of the sentences in this paragraph are not factual in nature. For example, the call for uninterrupted observations of the global climate system is a research recommendaiton that is outside the remit of this report and is not supported by text within the document. The same sentence also implies that current emissions scenarios are not "realistic", which is not a defensible statement given that historical emissions have tracked within the envelope of previous emission projections (see comment above and Figure 3.2.2), and argues that climate models should address biases and uncertainties, which is something that every climate modeling team already spends an enourmous amount of time doing. If retained, the non-scientific portions of this paragraph should be moved to the Preface, and the misleading statements should be corrected. The paragraph has been moved to the end of Chapter 12. See above regarding emission scenarios.

Preface	ix	1	The focus on "whether carbon dioxide emissions endanger the U.S. public" is slightly different from the stated focus in the Foreward on "the Nation's climate". This distinction is important because climate change and its impacts outside of U.S. borders can endanger the U.S. public both directly (e.g., global processes affecting sea level rise in the United States, or the impacts of climate change on overseas military installations) and indirectly (i.e., impacts of climate change on international agricultural production and trade dynamics). The reference to endangerment has been removed.
2	3	1	The two summary statements provide an extremely narrow and misleading view of "direct impacts of CO2 on the environment" (the chapter title). The most obvious direct impact of CO2, in the context of climate change, is its role in global radiative forcing, which is covered in the next chapter but not mentioned here. The chapter should at a minimum be retitled, but better yet combined with the following chapter which discusses other aspects of increasing CO2. The effect of CO2 on the climate takes up the bulk of the report.
2	3	2	It is misleading to state that ocean pH levels are within the range of natural variability. This statement is not true on the timescales governing the impacts of ocean acidification on ocean ecosystems, fisheries, and other processes. The evidence we cite shows that pH levels are in the range of natural variability. Whether the rate of change is, is much more difficult to establish because of the limitations of paleo records. Also the spatial heterogeneity is not subject to comprehensive measurement, which precludes straightforward assessment.
2	5	1	It is misleading to show (only) a graph of CO2 levels over hundreds of millions of years. These timescales are generally not relevant to processes and issues surrounding anthropogenic CO2 increases. For example the authors in multiple places make statements about CO2 levels during the last glacial maximum, but those fluctuations are undetectable on a graph with such a long timescale. The figure has been removed.
2	8	1	Acidification is not a misnomer...the word literally means to make something more acidic, which is what is happening. The authors may choose to argue that the term is misleading since the oceans are slightly basic, but that is not the same thing as a misnomer. We consider it a misnomer for the reasons stated. The oceans are not becoming acidic.
2	8	1	It is misleading and largely irrelevant to discuss ocean pH levels tens of thousands or millions of years ago, because that timescale is not relevant to the impacts associated with anthropogenic ocean acidification on ocean life. It is well documented that recent changes in ocean pH are happening ~100 times faster than natural fluctuations prior to human CO2 emissions, and marine species must adapt to changes in conditions on timescales of a few of their lifetimes, which is clearly not millions of years. The referee needs to provide specific sources for these statements. Also, it is a relevant point to make if current marine life evolved under conditions of much lower pH.
2	9	4	Similar to previous comment--changes in marine life over hundreds of millions of year has little relevance to the survival of marine species on decadal timescales due to anthropogenic ocean acidification. Are the authors trying to suggest that U.S. society would be well served by going back to conditions during the Jurassic? See earlier response.

3	13	2	The IPCC probably discounted the results reported in Connolly (2021) because that study included methodological errors that were documented in several subsequently publications, which the authors do not acknowledge. A serious assessment of TSI would not cherry-pick a single study, especially one that has been shown to include serious errors. The reviewer is probably referring to a comment by Richardson and Benestad, but Connolly et al. replied effectively to their critique.
3	14	4	It is unclear what the authors mean by "other natural sources of energy imbalance". If the authors are aware of a plausible additional source of energy imbalance they should state what it is rather than making vague statements without providing any evidence that such sources might exist. Surely the reviewer acknowledges that there are unknowns in the climate system
3	14	5	This is one of several places where the authors discuss the CO2 levels at which C3 plants begin dying in the context of the last glacial maximum. This is misleading, because there is no evidence that CO2 levels would have continued falling below the levels of the last glacial maximum, and irrelevant to the stated focus of this report, because they rebounded to 280 ppm many centuries before human activities began producing large amounts of CO2. The reviewer needs to provide evidence for the assertion that CO2 levels would not have continued falling.
3	15	1	The oceans and land surface absorb more like 60% of CO2 emissions. The 50% figure is repeated multiple times in the ensuing pages and should be corrected. We are unfamiliar with the 60% number; the sources with which we are familiar generally report numbers closer to 50%. The reviewer has not cited any sources for his or her statement.
3	20	2	The final sentence on this page is misleading and confusing as written. Most studies of the land carbon sink suggest that the rate at which it is increasing is declining, and several studies suggest that the ocean sink is declining. The authors should also explain the relevance of their statements here, because it isn't clear. Our statement is consistent with the sources we cite. Unfortunately the reviewer did not provide references.
4	26	6	The inclusion of a paragraph with high-level summary statements about climate policy here is confusing and inappropriate. This content belongs in chapter 11. We only make a brief introductory statement and otherwise defer the discussion to Ch 11.
4	29	1	The point about observed global temperature trends being consistent with both high-ECS high-aerosol and low-ECS low-aerosol worlds is an important one. It would be good to acknowledge that uncertainty in ECS is not just due to uncertainties in climate models, but also the lack of accurate measurements of pre-industrial aerosols and thus anthropogenic changes in aerosol forcing. And this uncertainty cuts both ways--if we live in a high-ECS world that has been "masked" by strong aerosol cooling, reducing air pollution could result in rapid warming (and this may be linked to the "hot model" problem as well). Agreed
4	29	2	The point about ECS values derived from past climate states (especially cooler ones) possibly not being totally representative of the current climate is another good point, one that would be good to acknowledge in their earlier and later discussions about uncertainty and the rationales for emissions reductions (e.g., on page 26 and in Chapter 11). Agreed.

4	30	1	The relatively shallow treatment on TCR relative to ECR here is puzzling, since the authors acknowledge that it is better constrained, and it is also a more relevant value for projected warming over the next several decades. The first sentence here also describes it as an observational constraint, but it is not, it is a metric that can be produced from either data or models (or both), and climate models are routinely used to run TCR experiments. ECS is more widely-used than TCR which is why we focus more attention on it.
5	32	1	A more accurate title of this chapter would be "deficiencies in climate models" because the authors focus almost exclusively on processes and features that models have struggled to reproduce, rather than a balanced assessment that also indicates what these models do well. We changed the title to take this into account.
5	34	1	Scaffeta (2023) has been widely criticized in the scientific literature for its methods and conclusions. These results should not be presented without acknowledging these concerns, or better yet, the authors should attempt to survey the literature on this topic rather than presenting results from a single controversial paper. The reviewer is referring to an exchange regarding a different paper. We are unaware of any published critiques of the Scafetta paper we cite.
5	34	2	Spencer (2024) is not a peer reviewed journal paper and is not really appropriate to include here when there are many papers on this topic that have been peer reviewed. The reviewer should have provided references. We are unaware of any peer reviewed papers that present the comparison we show. We provide traceable accounts for the underlying data, all of which is from official sources.
5	35	1	The statement that the signal for anthropogenic greenhouse warming emerges first and most strongly is not supported and is inconsistent with my understanding of climate dynamics. The following sentence is also dubious (and probably backwards) since models run with observed surface temperatures tend to produce much better mid-tropospheric temperature simulations. The statement about early and strong emergence of the CO2 signal in the tropical troposphere is in the IPCC TAR and subsequent reports. Even models constrained with observed sea surface temperatures produce excess amplification aloft.
5	36	1	In most places the authors correctly describe the discrepancy between models and observations as a mismatch, which is appropriate because observations of mid-tropospheric temperatures are also subject to uncertainty (as indicated in the error bars in Figure 5.4). This is one of the reasons for the IPCC confidence assessment noted in the following paragraphs. However, here and in a few other places in this section the writing implies that differences can be solely attributed to model deficiencies. We don't take a position on the ultimate cause however we do note that the biases are pervasive across models and multiple observational data sets.
5	37	1	This section should be combined with the previous one as it is really just elaborating on the same discrepancy between climate models and observational estimates of tropospheric temperatures. One section discusses the discrepancy of trends and the other discusses the discrepancy of amplification rates, which is why the sections are separate.

5	39	1	There has indeed been considerable controversy about this particular metric. It is curious that the authors choose to cite two older papers here (2008 and 2009) rather than the rich recent literature, especially since they went to the trouble of updating on of their graphics with 2024 data. Recommend including a more balanced assessment of this discrepancy, it's potential causes, and recent efforts to address it, as well as noting that climate models are able to accurately capture many other aspects of climate change. Again, it is unfortunate that the reviewer did not provide any citations to this rich recent literature. We cited the key papers we are aware of.
5	39	1	This entire section is very misleading as it cherry-picks a specific result and takes it out of important context. The reason why stratospheric cooling has slowed (and, at least in one study, even warmed) in the past two decades is due to the recovery of the ozone layer. This is clearly stated in the Philipona et al article cited by the authors and in the IPCC report. Additionally, satellite-based estimates of stratospheric temperature (which is data the authors highlight in the previous section) do not show the same trends. This section should be removed or amended to note that stratospheric temperatures are influenced by multiple factors and that a comprehensive analysis of stratospheric temprature trends is fully consistent with those projected under climate warming scenarios. We have added a sentence discussing the ozone recovery issue and make reference to the recent Santer et al. study.
5	41	1	Many observational studies have shown that snow cover is decreasing, and have explained why it is more meaningful and useful to look at spring trends in snow cover, as well as why observations of snow cover can be uncertain. The issue here is model projections versus observations. Again, the reviewer provides no citations.
5	42	4	The authors provide a relatively accurate survey of results regarding hurricane numbers and landfall frequency, which have indeed not shown any trends. But their analysis of hurricane intensity, which has been shown in many studies to increase, is lacking...the chart showing just estimated windspeeds of the top ten landfalling storms is really weak given the analytical talents of the author team. They also do not provide any discussion of the physical basis for why hurricane numbers may not be changing while the intensity of the strongest hurricanes is expected to increase, nor analyze any data in the context of those conclusions. The challenge for global intensity metrics is that the satellite record prior to 1980-1990 (experts disagree on the data quality during this period) is inadequate, which is why we focus on the Atlantic hurricane data. Also there is no theory for hurricane number; that is a topic associated with substantial uncertainty.
6	47	5	Long term persistence is not a recognized property or phenomenon associated with rainfall data. The use of such a statistical technique, especially one that is not widely recognized or used in the hydroclimate community, demands additional justification. We have rewritten the text to remove specific reference to LTP.

6	53	4	The authors have cherry picked results throughout this section. For example, they have selected only a few quotes from IPCC and NCA describing extreme heat. The absolute single daily maximum temperature for a given year, for example, is a poor statistic for assessing statistical changes in extreme heat (it would be like comparing the tallest person in each country to try to calculate change in average height). A complete and comprehensive review of the underlying assessments and the data it cites clearly shows that extreme heat has been increasing in North America. The reviewer fails to identify any data or conclusion from the IPCC or NCA that differs from our assessment, and fails to cite any sources to justify this comment.
6	56	1	The "climate less prone to extremes" is a misleading statement. If minimum temperatures are increasing faster than maximum temperatures (which is an expectation of climate change based on basic physical principals), then the difference between warm and cold extremes on many timescales is expected to decrease. There are reasonable points to be made regarding observational deficiencies, regional variations in extreme events, and the potential benefits of reductions in extreme cold, rather than coming up with misleading statistics. But we show that extreme heat has also declined since the 1930s – Figures 6.3.1 and 6.3.3 refute the reviewer's statement.
6	58	3	The NCA5 heading is not a controversial statement, it simply says that things are changing, which they are by the author's own admissions and graphs. It does not suggest pervasive positive trends, as the authors suggest it does. We're not sure what sentence the reviewer is referring to.
6	61	3	The statistical techniques used in the 2019 paper are highly non-standard for evaluating precipitation statistics, and the 2024 results have not been peer reviewed. The results reported in the NCA and IPCC and the underlying studies have clearly demonstrated that extreme precipitation is increasing. The 2019 paper uses ordinary least squares to compute the trend, and a standard variance estimator from the econometrics literature to assess significance. As we point out, we also find evidence of increasing precipitation averages and extremes but only when selecting certain start dates.
6	61	7	The relevance of the paragraph on 19th century and paleoclimatic rainfall events is unclear. If anything, the fact that such extremes have occurred in the past argues for more aggressive adaptation measures to ensure that damages won't be as high if they happen again (even notwithstanding potential changes in risk due to climate change). Note, however, that additional context is provided in the box that follows two pages later. We agree that discovery of extreme events in the 19th century records indicates that disaster planning may need to encompass a wider range of risks.
6	62	1	The choice of looking at (only) 5-day precipitation totals is questionable. Most rainfall events, including the most extreme and damaging precipitation events, are typically on a daily timescale. Again, the reviewer provides no citations or evidence. Our results are robust to different day intervals, and we refer to 1- and 2-day results as well.
6	68	3	Perhaps this is more germane to the sea level rise section, but nuisance (high tide) flooding due to sea level rise has been observed across much of the United States. This is a topic we did not address.

From: John Christy <climateman60@gmail.com>
Sent: Monday, August 4, 2025 12:04 PM
To: Ross McKittrick; Steve Koonin; Roy Spencer; Judith Curry
Subject: DoE Reviewer #6 response edits

To the DoE Reviewer Curator

Reviewer #6

5 34 4 “... are widely used models from ...”

5 39 3 “... but does reluctantly note the discrepancy.”

5 39 3 “The amplification discrepancy was briefly noted in AR5 (9.4.1.4.2) and not discussed in AR6” [Delete: I don’t think AR5 assessed the amplification discrepancy except maybe to dismiss it. But AR6 should have in any case.]

5 40 1 “Santer et al. (2023) has been added”. [for CWG: Santer et al. 2025 was a different paper in which they claim that the fingerprint method would have found anthropogenic climate effects as early as the late 19th century]

6 61 3 [for CWG: question as to whether to add these three new areas to address the criticism of “cherry-picking”. At the time of the CWG deadline, I had only built time series for the three regions we presented.]

John C.

From: John Christy <climateman60@gmail.com>
Sent: Thursday, August 14, 2025 1:15 PM
To: Judith Curry
Cc: Steve Koonin; Ross McKittrick; Roy Spencer
Subject: Re: Hurricane section

Judy

This reference list is quite revealing - the citations not mentioned in IPCC in particular due to new results. I wonder how many of our CWG references were not included in the thousands listed in WGI of IPCC?

I've been communicating with Dessler directly now. With me, they are asking questions about Chapter 6. They are grateful for our posting of the datasets and for the ancillary datasets I've sent that went into our analysis. So far, questions are pretty standard - the kinds Roy and I have dealt with all our lives (did you do areal-weighting, how to handle TOBs issues, etc.).

John C.

On Wed, Aug 13, 2025 at 4:55 PM Judith Curry <curry.judith@gmail.com> wrote:
Excellent that you are working with Dessler et al

Courtesy of Grok3

Cited in IPCC AR6 (WGI, primarily Chapter 11):

- Walsh et al. (2015)
- Kim and Ho (2018)
- Knaff et al. (2014)
- Kossin (2018)
- Lanzante (2019)
- Hall and Kossin (2019)
- Landsea and Franklin (2013)
- Bhatia et al. (2019)

Not Cited in IPCC AR6:

- Forbis et al. (2024): Post-cutoff
- Soule et al. (2012): Regional focus, not in bibliography
- Kellner et al. (2016): Regional focus, not in bibliography
- Aryal et al. (2018): Not in bibliography
- Guzman and Jiang (2021): Post-cutoff
- Tu et al. (2021): Post-cutoff
- Wang et al. (2025): Post-cutoff
- Fritz (2009): Thesis, not typically cited
- Kossin (2019): Reply, not primary research
- Garner (2023): Post-cutoff
- Balaguru et al. (2024): Post-cutoff
- Jewson and Lewis (2020): Not in bibliography

Notes

- The assessment is based on publication dates, relevance to AR6 WGI Chapter 11, and checks against available bibliographies. Some papers may not be cited due to regional focus, journal prominence, or overlap with more comprehensive studies.
- For papers published in 2021 or later, the AR6 cutoff (mid-2020) excludes them automatically.
- If you need further details or want me to check specific AR6 chapters beyond WGI, let me know!

On Wed, Aug 13, 2025 at 2:18 PM John Christy <climateman60@gmail.com> wrote:
Excellent Judy. Were many of the newly included studies cited in the IPCC?

I'm having conversations with the Dessler group ... being completely helpful with data and methods used in Chapter 6.

John C.

On Wed, Aug 13, 2025 at 3:13 PM Judith Curry <curry.judith@gmail.com> wrote:
See attached revisions to the hurricane section. I think this strengthens the text without diluting our main points. I look forward to your feedback/comments.

--



Judith Curry, President
CFAN - Climate Forecast Applications Network
Reno, NV USA
curry.judith@cfanclimate.com | +1.404.803.2012
<http://www.cfanclimate.net>



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curry.judith@cfanclimate.com | +1.404.803.2012
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Sent: Wednesday, August 13, 2025 5:55 PM
To: John Christy
Cc: Steve Koonin; Ross McKittrick; Roy Spencer
Subject: Re: Hurricane section

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On Wed, Aug 13, 2025 at 3:13 PM Judith Curry <curry.judith@gmail.com> wrote:

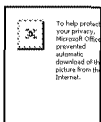
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Judith Curry, President
CFAN - Climate Forecast Applications Network
Reno, NV USA
curry.judith@cfanclimate.com | +1.404.803.2012
<http://www.cfanclimate.net>

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Judith Curry, President
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Cc: Steve Koonin; Ross McKittrick; Roy Spencer
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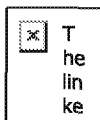
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CFAN - Climate Forecast Applications Network
Reno, NV USA
curry.judith@cfanclimate.com | +1.404.803.2012
<http://www.cfanclimate.net>

From: Judith Curry [curry.judith@gmail.com]
Sent: 8/13/2025 8:13:08 PM
To: Steve Koonin [steven.koonin@gmail.com]; Ross McKittrick [ross.mckittrick@gmail.com]; John Christy [climatemanager60@gmail.com]; Roy Spencer [roywspencer@hotmail.com]
Subject: Hurricanes section
Attachments: DOE section 6.2 (2).docx

See attached revisions to the hurricane section. I think this strengthens the text without diluting our main points. I look forward to your feedback/comments.

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Judith Curry, President
CFAN - Climate Forecast Applications Network
Reno, NV USA
curry.judith@cfanclimate.com | +1.404.803.2012
<http://www.cfanclimate.net>



6.2 Hurricanes and tropical cyclones

AR6 provides the following assessment of tropical cyclones (TCs; used here as a synonym for hurricanes):

AR6: There is *low confidence* in most reported long-term (multidecadal to centennial) trends in TC frequency or intensity-based metrics due to changes in the technology used to collect the best-track data. (IPCC, 2021 p. 1585)

AR6: It is *likely* that the global proportion of major (Category 3–5) tropical cyclone occurrence has increased over the last four decades . . . There is *low confidence* in long-term (multi-decadal to centennial) trends in the frequency of all-category tropical cyclones. (IPCC, 2023 SPM p. 9)

AR6: A subset of the best-track data corresponding to hurricanes that have directly impacted the United States since 1900 is considered to be reliable, and shows **no trend in the frequency of U.S. landfall events**. (IPCC 2021 p. 1585)

6.2.1 Observations of tropical cyclone frequency and intensity

Since 1980, when satellite observations first fully covered the global oceans, we have confidence in the numbers of total global hurricanes and major hurricanes (Category 3 and higher). Figure 6.2.1 shows that on average, each year there are about 50 hurricanes, with about 25 reaching major hurricane status (Maue, 2025). There is substantial year-to-year and decadal variability, a weak decrease in the number of hurricanes, and a slight but insignificant increase in the number of major hurricanes. These two trends combine to create an overall increase in the proportion of major hurricanes. Jewson and Lewis (2020) assess this global trend to be associated with a decrease in Category 1 storms outside the North Atlantic and an increase in Category 3 and 4 hurricanes in the North Atlantic since 1995.

Global hurricane statistics are dominated by the Northwest Pacific Ocean, which accounts for ~35 percent of total global hurricanes, whereas the Atlantic accounts for ~15 percent of global hurricanes (Colorado State University, 2025). Data in the Atlantic basin extends further back than in the other ocean basins and is most relevant to U.S. policy makers.

Global Major Hurricane Frequency -- 12 month running sums -- @RyanMaue

Updated March 10, 2025

Last 30-years, annual: 45 H | 24 M

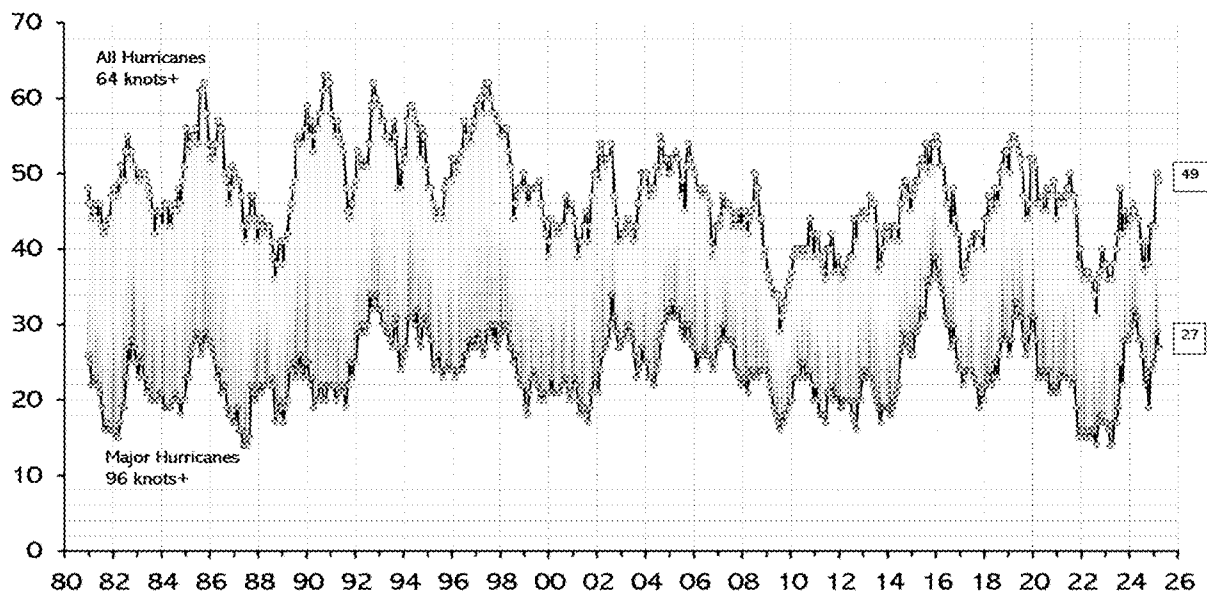


Figure 6.2.1: Global frequency of hurricanes and major hurricanes since 1980. Source: Updated from Maue 2011.

Figure 6.2.2 shows the frequency of Atlantic hurricanes and major hurricanes (Category 3 and higher) back to 1920. Data prior to 1965 (the onset of satellite observations in the Atlantic, shaded in blue) shows some undercounting, with data prior to 1920 showing substantial undercounting (Vecchi and Knutson, 2011). All measures of Atlantic hurricane activity show a significant increase since 1970. However, the period from 1971-1994 saw exceptionally low activity, with high activity (comparable to the past two decades, even with undercounting) also observed during the 1950's and 1960's, and even in the 1930's.

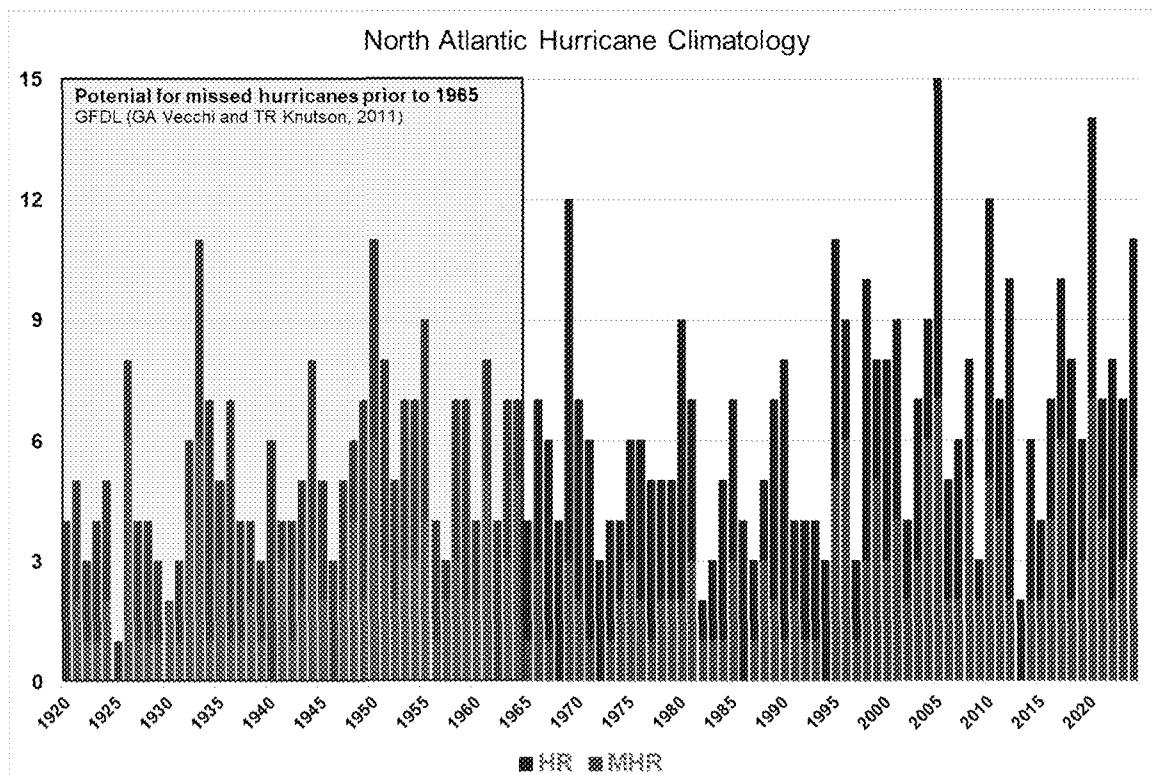


Figure 6.2.2: Atlantic frequency of hurricanes (HR) and major hurricanes (MHR) since 1920. Source National Hurricane Center (2024)

Figure 6.2.2 shows that Atlantic hurricanes vary strongly on decadal and multidecadal time scales. These variations are associated primarily with the Atlantic Multidecadal Oscillation (AMO), which is manifest in basin-wide sea surface temperature and sea level pressure fluctuations connected to large scale ocean circulation patterns. The AMO was in its warm phase during 1926-1970 and 1995-present, but in its cool phase during 1971-1994. It has its greatest impact on the number of major hurricanes (Category 3+), which Goldenberg *et al.* (2001) associated with above normal SSTs and decreased vertical shear in the AMO warm phase (see also Bell and Chelliah, 2006; Klotzbach *et al.*, 2018).

Klotzbach *et al.* (2018) conducted a comprehensive evaluation of the landfalling hurricane data for the Continental U.S. since 1900. Figure 6.2.3 updates their analysis through 2024. While the largest numbers of landfalling hurricanes are from 2004, 2005 and 2020, there is no statistically significant trend since 1920. Figure 6.2.3 also shows the time series for major hurricane landfalls (Category 3-5). The largest year in the record is 2005, with 4 major hurricane landfalls. However, following 2005 there were no major hurricanes striking the U.S through 2016, the longest such period since 1920.

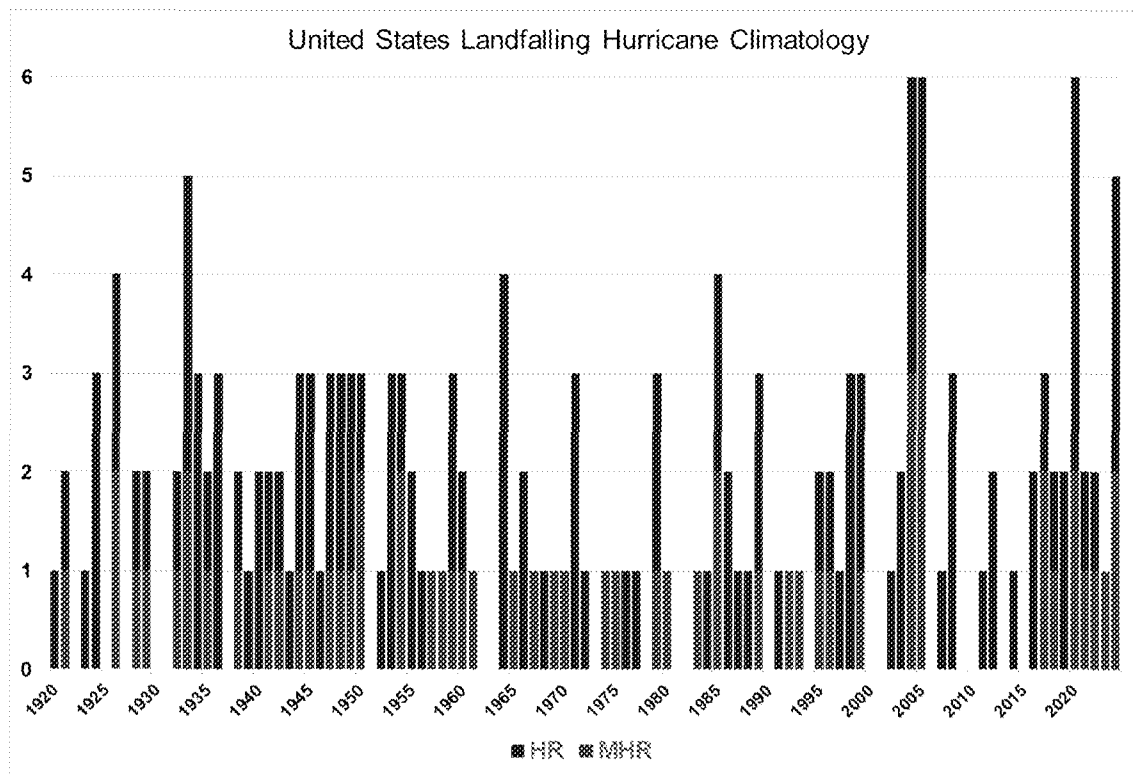


Figure 6.2.3: U.S. landfalling frequency of hurricanes (HR) and major hurricanes (MHR) since 1920. Source NOAA HRD(a) (2024)

Figure 6.2.3 shows substantial interannual to multidecadal variability in U.S. landfall activity. Klotzbach *et al.* (2018) examined how the landfall counts vary with ENSO (El Niño versus La Niña) and the warm versus cold phases of the Atlantic Multidecadal Oscillation (AMO). Villarini *et al.* (2012) provide an analysis of U.S. hurricane landfalls back to 1878. While it is possible that some landfalls were missed in the late 19th century owing to sparsely populated regions on the Gulf Coast, it is remarkable that the highest year in the entire record is 1886, with 7 hurricane landfalls, when human influences on the climate were much smaller than they are today.

In summary, analyses of both global and regional variability and trends of hurricane activity provide the basis for detecting changes and understanding their causes. The relatively short historical record of hurricane activity, and the even shorter record from the satellite era, is not sufficient to assess whether recent hurricane activity is unusual relative to the background natural variability.

6.2.1 Factors contributing to U.S. economic losses

Economic losses from landfalling hurricanes have increased in recent decades, both in the U.S. and globally. Any signal from global warming is confounded by economic development and population increase. Identifying a signal from human caused global warming in the increased losses requires identifying a trend that can be attributed to global warming in any of the factors that contribute to economic losses from landfalling hurricanes. Characteristics of an individual

storm that produce damaging winds, storm surge, tornadoes, and flooding from rainfall include: intensity (maximum wind speed), horizontal storm size, and track speed near the coast. We focus here on the observational record, and describe theoretical support for interpreting any trends.

Intensity and rates of intensification. With regards to intensity (maximum wind speed), Table 6.2.1 shows the 10 strongest hurricanes (plus ties) to make U.S. landfall. Of the hurricanes that have made landfall with sustained winds greater than 150 mph, only one has occurred in the 21st century. However, note that since 2020 there are three hurricanes in this group with landfall winds of 150 mph.

Rank	Year	Landfall Wind (MPH)	Name
1	1935	185	"Labor Day"
2	1969	175	Camille
3	1992	165	Andrew
4	2018	160	Michael
5	1856	150	"Last Island"
5	1886	150	"Indianola"
5	1919	150	-----
5	1932	150	"Freeport"
5	2004	150	Charley
5	2020	150	Laura
5	2021	150	Ida
5	2022	150	Ian

Table 6.2.1 Strongest hurricanes to make landfall along the U.S. coast. Source (NOAA HRD(b), 2024)

While it has long been hypothesized that a rising global sea surface temperature would cause an increase in hurricane intensity, identification of any significant trend in the hurricane data is hampered by a short data record, lack of precision and uncertainty in wind measurements, and substantial natural variability. The precision of intensity measurements is 5 knots (5.7 mph). Landsea and Franklin (2013) assessed that the uncertainty in satellite-only intensity observations (which is the case for most of the globe) is about 15% for category 1 and 2 hurricanes and 10% for major hurricanes (Category 3+). For aircraft/satellite monitoring and for U.S. landfalling cyclones, the relative uncertainty decreases to about 10% for category 1 and 2 hurricanes and 8% for major hurricanes. Since 2000, new satellite and aircraft-based remote sensing systems have lowered uncertainty in the intensity estimates.

There is some evidence suggesting that the *rate* of intensification (if not peak intensity) has increased with warming. If rapid intensification occurs near the coast, society is vulnerable because there is less time to prepare and evacuate. Garner (2023) identified a substantial increase from a historical era (1970-1990) to the modern era (2001-2020) in the likelihood for a tropical cyclone in the Atlantic to rapidly intensify. Bhatia et al. (2019) analyzed global rates of hurricane intensification for the period 1982-2009. They also found a significant increase in the Atlantic, but globally the signal was ambiguous with different data sets disagreeing on whether or not

there was a trend. Balaguru et al. (2024) identified a global increase in nearshore tropical cyclone intensification rates. Natural climate modes, such as the Atlantic Multidecadal Oscillation (AMO) shifting to a positive phase in the late 1990s or the Pacific Decadal Oscillation (PDO), correlate with much of the observed variability in intensification rates. Uncertainties in intensity measurements imply low confidence in these findings.

Track speed. Slower track forward speed is associated with larger storm surges and large rainfall accumulations. Kossin (2018) showed that that tropical-cyclone translation speed (rate of forward motion) has decreased globally by 10% over the period 1949-2016. The largest slowdown was found in the western North Pacific Ocean and the region around Australia. With regards to the North Atlantic, Hall and Kossin (2019) identify increasing frequency of slowdown or direction reversal near the coast. There is low confidence in these analyses owing to data heterogeneities, particularly with the introduction of satellite data (Lanzante et al. 2019; see also Kossin 2019 reply).

Hurricane size. Hurricane size impacts storm surge, the number of tornadoes, and the amount of rainfall. Horizontal size was particularly important in the impacts of Hurricanes Katrina (2005) and Sandy (2012). Relatively little research has been done on climatic variations of hurricane size. A satellite-based hurricane size climatology was developed by Knaff (2014). Some limited information on the variability of Atlantic hurricane size is provided by Fritz (2009). While substantial year-to-year variability is seen, a large jump occurs in 1995, correlated with the transition to the warm phase of the Atlantic Multidecadal Oscillation (AMO). Wang et al. (2025) provides evidence that tropical cyclone size does not change strongly with mean warming, but expands faster over warmer relative sea surface temperatures.

Storm surge. The magnitude of a storm surge depends on storm intensity, forward speed, horizontal size, angle of approach to the coast, central pressure, tidal fluctuations, and the shape and characteristic of coastal features. The slow creep of sea level rise (Chapter 7) is contributing a small amount to the magnitude of hurricane-induced storm surges. Depending on local topography, a small change in sea level can translate into an increase in the inland reach of the storm surge. As summarized by Knutson et al. (2019), our expectation is that some worsening of total inundation levels during storms is occurring globally associated with the anthropogenic contributions to global and regional sea level rise, assuming all other factors equal. However, a climate change signal has not yet been convincingly identified as a residual in sea level extremes data, after accounting for background sea level rise.

Rainfall. It has long been hypothesized and predicted that hurricane-induced rainfall would increase in a warmer climate (see section 11.7 of AR6 WGI Report). However, the assessment by Walsh et al. (2015) concluded that for the globe, a detectable change in hurricane-related rainfall has not been established by existing studies. Satellite data is being increasingly used to assess tropical cyclone rainfall. Kim and Ho (2018) examined the variation of hurricane rainfall area over the subtropical oceans using satellite radar precipitation data collected from 1998 to 2014. In the subtropics, higher translation speed and larger vertical wind shear significantly contribute to an increase in hurricane rainfall area by making horizontal rainfall distribution more asymmetric, while sea surface temperature rarely affects the fluctuation of hurricane rainfall area. They suggested that in the

subtropics, unlike the tropics, atmospheric circulation conditions are likely more crucial to varying hurricane rainfall area than factors such as sea surface temperature.

Tu et al. (2021) and Guzman and Jiang (2021) showed that the tropical cyclone rain rate had increased globally during the period 1999–2018, primarily due to an overall increase in the outer band of tropical cyclones, as opposed to in the inner band where the rain rate had decreased considerably. These trends were associated with increases in sea surface temperature and total precipitable water in the tropical cyclone environment.

Specifically with regards to the U.S., Soule et al. (2012) estimated that tropical cyclones contribute about a quarter of the annual rainfall in the southeast U.S. Between 1950 and 2008, the majority of counties in Florida, Georgia, South Carolina and North Carolina saw at least 20% of their droughts ended by a tropical cyclone. Hurricanes also account for approximately 20% of the observed monthly rainfall from June to November across the eastern U.S. Corn Belt (Wisconsin, Michigan, Illinois, Indiana, Ohio and Kentucky) (Kellner et al. 2016).

Several factors have led to excessive rainfall from Atlantic landfalling hurricanes. Slow motion of the hurricane near landfall can lead to high amounts of local rainfall—Danny (1997), Wilma (2005) Harvey (2017); Florence (2018). Mountains/hills near the coast magnify rainfall potential due to forced upslope flow – Mitch (1998). Upper-level troughs and cold fronts can lead to excessive rainfall –Floyd (1999). Horizontally large hurricanes have larger rain footprints, which can lead to excessive local rainfall accumulations owing to the longer time frame over which rainfall falls at any one location. In the U.S., the record hurricane rainfall is from Hurricane Harvey (2017), which was caused by stalling of the hurricane on the Texas coast.

Aryal et al. (2018) examined the contribution of North Atlantic hurricanes to flooding and heavy rainfall across the continental U.S. Their analysis does not point to any significant changes in the magnitude or frequency of floods. They found that the North Atlantic Oscillation (NAO) plays a significant role in hurricane-related extreme rainfall along the U.S. East Coast, while ENSO is most strongly linked to the hurricane-related precipitation in Texas.

In summary, there is no clear signal of increasing rainfall from U.S. landfalling hurricanes owing to the complexity of the factors contributing to hurricane-induced rainfall.

Tornadoes. The climatology of hurricane-induced tornadoes has received little attention. It has been hypothesized that hurricanes might be capable of producing more tornadoes with warming (Forbis et al. 2024), although there has been no detection in historical data.

Summary. All measures of Atlantic hurricane activity show a significant increase since 1970. However, high values of hurricane activity (comparable to the past two decades) were also observed during the 1950's and 1960's, and by some measures also in the late 1920's and 1930's. Atlantic hurricane processes are influenced substantially by the natural modes of ocean circulation variability in the Atlantic, notably the Atlantic Multidecadal Oscillation. Hence there is low confidence in any claims that human-caused warming is worsening U.S. impacts from landfalling hurricanes.

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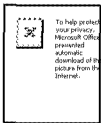
From: Judith Curry <curry.judith@gmail.com>
Sent: Saturday, August 16, 2025 11:38 AM
To: Steven Koonin
Cc: John Christy; Roy W. Spencer; Ross McKittrick; Travis Fisher
Subject: Re: Finally, an Unbiased and Objective Climate Science Report | RealClearScience

This is excellent, best article so far

On Sat, Aug 16, 2025 at 8:17 AM Steven Koonin <steven.koonin@gmail.com> wrote:

https://www.realclearscience.com/articles/2025/08/16/finally_an_unbiased_and_objective_climate_science_report_1129196.html

Steven E. Koonin



Judith Curry, President
CFAN - Climate Forecast Applications Network
Reno, NV USA
curry.judith@cfanclimate.com | +1.404.803.2012
<http://www.cfanclimate.net>

From: Steven Koonin <steven.koonin@gmail.com>
Sent: Saturday, August 16, 2025 11:17 AM
To: John Christy; Roy W. Spencer; Judith Curry; Ross McKittrick; Travis Fisher
Subject: Finally, an Unbiased and Objective Climate Science Report | RealClearScience

https://www.realclearscience.com/articles/2025/08/16/finally_an_unbiased_and_objective_climate_science_report_1129196.html

Steven E. Koonin

From: Ross McKittrick <ross.mckitrick@gmail.com>
Sent: Saturday, August 16, 2025 11:25 AM
To: John Christy
Cc: Roy Spencer
Subject: Re: Dessler Sec 6.3

Have they published this claim anywhere?

And how robust is it to different metrics? Tony Heller frequently shows different measures of daily exceedances and the 1930s always stands out.

It seems to me that at worst what we would show is the current understanding based on NCA4 and NCA5 style analyses both of which point to peaks in the 1930s (though not stated clearly in NCA5 as I recall) then say it's been challenged by ZH assuming he's published his findings somewhere.

On Sat, Aug 16, 2025 at 8:18 AM John Christy <christy@nsstc.uah.edu> wrote:

Ross

Zeke is claiming, as best I can tell, that due to various errors in the USHCN data, that current warming is pretty much as bad as it was in the 1930s. I just received this last night (the part about Zeke's chart), but I think he (or Andy) is committing a fundamental methodological error - we are dealing with daily extremes and those daily extremes are not impacted by the TObs error. The two charts I sent demonstrate that (using metrics that have zero TObs influence) the picture is the same as we presented in 6.3.

John C.

John R. Christy
Director, Earth System Science Center
Professor, Atmos and Earth Science
Alabama State Climatologist
The University of Alabama in Huntsville
christy@nsstc.uah.edu

On Aug 16, 2025, at 9:51 AM, Ross McKittrick <ross.mckitrick@gmail.com> wrote:

I'm just seeing this for the first time. Is their contention that the NCA4 graph showing max heat wave activity was in the 1930s has been superseded and the max is now in the recent part of the record?

On Sat, Aug 16, 2025 at 1:23 AM Roy Spencer <roywspencer@hotmail.com> wrote:
John:

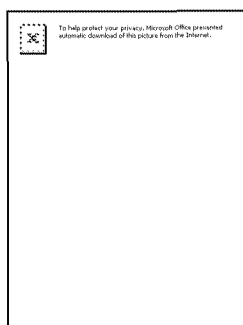
(I'm ccing Ross since he's good at knowing how to phrase responses to criticisms of the report).

So, you used raw USHCN data for those calculations (like the heat wave statistics)? If so, I think we can be dinged for not using the homogenized dataset.

Our paper on UHI only estimated how large the UHI effect was on US Tavg temperatures from the RAW GHCN data, a change which was required by one reviewer, which I think was probably Hausfather. Now, it's interesting that the UHI magnitude results were nearly the same as when I used homogenized dataset, which I can't explain. That result was not mentioned in the paper because I'm sure the reviewer would have demanded it be removed.

What might help is the recent paper published arguing that the homogenization process tends to make rural stations look like urban stations (the opposite of what is required), the so-called "urban blending" problem, which casts doubt on the whole homogenization procedure. The most recent and probably the most in-depth analysis was by Katata et al.

<https://journals.ametsoc.org/view/journals/apme/62/8/JAMC-D-22-0122.1.xml>



Evidence of Urban Blending in Homogenized Temperature Records in Japan and in the United States: Implications for the Reliability of Global Land Surface Air Temperature Data in: Journal of Applied Meteorology and Climatology Volume 62 Issue 8 (2023) - AMETSOC

Abstract To reduce the amount of nonclimatic biases of air temperature in each weather station's record by comparing it with neighboring stations, global land surface air temperature datasets are routinely adjusted using statistical homogenization to minimize such biases. However, homogenization can unintentionally introduce new nonclimatic biases due to an often-overlooked statistical ...

journals.ametsoc.org

Maybe you could argue that such recent studies cast doubt on the homogenized data, so you used raw data instead? Is there something specific I can do in my new calculations to help?

Or, just say we will examine the issue as part of the report changes, and avoid making hasty judgements for now?

-Roy

From: John Christy <christy@nsstc.uah.edu>
Sent: Friday, August 15, 2025 10:22 PM
To: Roy Spencer <roywspencer@hotmail.com>
Subject: Fwd: DoE Report

Roy

I think these are the generic USHCN adjustments which I believe you've shown are biased warm? I'll wait to hear from you before answering Andy.

John C

John R. Christy
Director Earth System Science Center
Alabama State Climatologist
Professor Atmospheric and Earth Science
The University of Alabama in Huntsville
256 961 7763

Sent from my iPhone

Begin forwarded message:

From: Andrew Dessler <adessler@tamu.edu>
Date: August 15, 2025 at 10:09:40 PM CDT
To: John Christy <christy@nsstc.uah.edu>, Zeke Hausfather
<hausfath@gmail.com>, John N-G <n-g@tamu.edu>
Subject: Re: DoE Report

John: Thanks for your answer. I asked Zeke Hausfather (cc'ed) about biases in the USHCN data and he sent me a plot:
<[A7B7AE5C-8A5A-4A55-90E6-02BA3B9902D1.png](#)>

This shows monthly USHCN raw and bias corrected temps and it certainly looks to me like the biases are big enough to have a large impact on the ranking of the years.

Given this, you're standing by the conclusions of Section 6.3? Don't you think that, given the importance of this report, you should do more work to quantitatively show that the biases are unimportant? What you described in your previous emails is really an anecdote + hunch. I believe that, if I used the same level of arm-waving to argue that climate models were accurate, you would object strongly.

Thanks,
Andy

Andrew Dessler
Professor of Atmospheric Sciences
Director, Texas Center for Extreme Weather

Texas A&M University
adessler@tamu.edu
website: <https://artsci.tamu.edu/atmos-science/contact/profiles/andrew-dessler.html>

On Thu, Aug 14, 2025 at 10:15 AM John Christy <christy@nsstc.uah.edu> wrote:

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Hello Andy

This section was shortened quite a bit from earlier drafts. I suppose the easiest reference would be Mass et al. 2024 which makes the point the variability of daily extremes (order 30 to 40 °F relative to the “normal”) is well over an order of magnitude greater than that which might be expected from GHG forcing (1 or 2°F as stated in the paper). This works best for TMax (daily variability much greater than GHG and less influence of UHI) rather than TMin. TMin has risen almost universally in the data related to the expanding presence of manufactured surfaces around the stations (sort of UHI effect as noted in Spencer et al. 2025, which is cited in CWG, pointing out that the greatest impact is when the station first starts to experience changes from pristine to slightly rural). Additionally the nocturnal boundary layer, which is critical for the TMin value, is significantly affected when only small changes in material surfaces and roughness occur (see description in Christy et al. 2009 pp 3353-54 and for theory/modeling see McNider et al 2012 which were referenced in the CWG report.) So putting together Mass et al. 2024, Spencer et al. 2025 and Christy et al. 2009 (and others) the evidence supports this conclusion.

In an interesting result, Kim and Christy 2022 go into detail for one station (and stations surrounding it) showing the existence of rapid TMin warming relative to virtually none in TMax despite the major UHI influence (see Fig. 7). The interesting point is that once adjusted for many issues (that even Berkeley Earth or NCEI don't do) the final TMax time series was very very close to the unadjusted TMax time series. We've seen this elsewhere too - that the daytime mixing of the deeper atmosphere tends to negate the near-surface local effects of UHI which are profound for TMin ... and, the other changes (instrumental, location, time-of-obs, etc.) tend to just cancel each other out over the entire time series for TMax. Note we did not use this dataset for trend estimation of absolute temperatures - the adjustments would be necessary to pull out the small, better characterized trends. Note too, the use of hundreds of stations knocks down the random noise (changes in location, instrumentation etc.) to give a good signal.

I think a bottom line, as we note, is that no matter how you slice and dice the data, there were some tremendous heat waves in the CONUS (mostly Central and East) from 1925-1954 and most recently (2013-2024) in the West. The 1960s and 1970s were not hot at all. The real implication, which we did not

address because there was a policy flavor to it, is that being prepared for what we know has already happened will go a long way to building resilience for anything in the future, especially for hydrologic extremes.

So, that's a lot to say to indicate that for TMax extremes, the USHCN dataset will be informative, but TMin will likely be at least slightly affected by the UHI effect as we state in 6.3.1. Let me know if you need more information - we are happy for your help in making this report represent these particular issues in the clearest way possible (we didn't address everything of course).

Thanks.

John C.

John R. Christy
Distinguished Professor, Atmospheric and Earth Sciences
Director, Earth System Science Center
Alabama State Climatologist
The University of Alabama in Huntsville
256-961-7763

On Aug 13, 2025, at 10:06 PM, Andrew Dessler
<adessler@tamu.edu> wrote:

Hi John (cc: N-G): I hope you don't mind, but N-G forwarded me your response because I was curious about the methodology. Thanks for confirming that you gridded the results to adjust for the non-uniform coverage of the USHCN data — that was definitely a question in my mind.

One remaining question is the origin of the statement in your report that the USHCN data, despite having well known issues in the daily data (b/c it's not homogenized), is nevertheless suitable for these calculations (Figs. 6.3.3-6.3.6). There was no reference or other explanation and it's not at all obvious to that's true. I'm wondering what that's based on.

Thanks in advance for your help on this question.

Andrew Dessler
Professor of Atmospheric Sciences
Director, Texas Center for Extreme Weather
Texas A&M University
adessler@tamu.edu

website: <https://artsci.tamu.edu/atmos-science/contact/profiles/andrew-dessler.html>

On Wed, Aug 13, 2025 at 3:37 PM John N-G <n-g@tamu.edu> wrote:

----- Forwarded message -----

From: **John Christy** <christy@nsstc.uah.edu>

Date: Wed, Aug 13, 2025 at 3:15 PM

Subject: Re: DoE Report

To: John N-G <n-g@tamu.edu>

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John

I'm afraid the code is my version of FORTRAN, but since these analyses were simple arithmetic, the analyses should be easily reproducible and I would be very interested if you produced the same values. A couple of files needed would be the half-degree grid mask for the CONUS and the corresponding regional identifier (attached). The txt files are 116 x 50 with 0 meaning no part of CONUS there. You can see from the map (Fig. 6.3.2) that a simple average of stations will cause problems since there are some pretty dense clusters and fairly wide-open places, hence the areal weighting. Left lower corner is -124.75, 25.25 degrees Lon/Lat in the files once read in.

```
C  read CONUS grids usa48_halfdeg.txt.  nx = 116, ny =
50. First row is northern most row, so I read the N-S rows in
reverse order to put N at the top.
```

```
c
```

```
do iy = ny,1,-1
  read (35,*),(nus48(ix,iy),ix=1,nx)
end do
```

```
C  read region identifier in userg_half.txt
```

```
do iy = ny,1,-1
  read (36,*) (nusreg(ix,iy),ix=1,nx)
end do
```

After the CONUS is gridded for a particular variable, I use the regional identifiers to calculate the regional values with cosine(latitude) weighting.

Here is the bit that does the counting of values above “nthresh” which in this case was 95F where ival is the full array of data for a station, for months 5 to 9 (May to Sep), iyr goes from 1899 to 2024. Pretty straight forward. For each station (istn):

```

do iyr = nyb,nye
  kttotal = 0
  kday = 0
  do imo = nmon1,nmon1 + nmon-1
    do idy = 1,31
      if( ival(iyr,imo,idy) .gt. -900) then
        if( ival(iyr,imo,idy) .ge. nthresh) then
          kttotal = kttotal + 1
        end if
        kday = kday + 1
      end if
    end do ! idy
  end do ! imo
  if( kday .gt. 120) then
    xrec(iyr,istn) = kttotal
  else
    xrec(iyr,istn) = -999
  end if
end do ! iyr
print *,nst(istn),kttotal,kday

```

So for example, in calculating the number of 95F days in the tmax file (Fig. 6.3.5), for each station I counted the number of non-missing days in each May-Sep by year, and if the number was > 120. (Out of 153 possible) then I counted the number of those greater than 94F. When all stations were completed for all years, I went back and calculated a value for each half-degree grid using a radius of 125km for the station’s information to be included in the value of that grid by a $1/d^2$ factor (d is distance from the center of the grid to the station - the station locations are in the station list file on my public account). This radius covered 99% of the actual CONUS area (a few places in TX and NV couldn’t be reached with station data). I compared my interpolation method with a python mapping method and they produced results virtually identical.

Note that in the datafiles, the stations are all identified by their original COOP IDs rather than by WBAN which was used for about 80 stations as provided by NCEI.

I went through lots of parametric variations with the threshold figures and other heat indices, and the same answer came out each time - the CONUS was hot from 1925-1954, got cool,

then has warmed in the last few years, but mostly in the West. In fact, the West has experienced their "dust bowl" in the past roughly 10 years in the sense their proportion of heat-wave days is similar to that of the Central-East in the 1930s.

For Fig. 6.3.4, I calculated the hottest (coldest) values for warm (cold) season by years and station as a departure from the median from all years of the hottest (coldest) temperatures. These "anomalies" were then mapped into the CONUS grid, geographically averaged, then subtracted to get the difference between the single hottest and coldest day.

Fig. 6.3.6 required a little more effort as heatwave days required a test of consecutive values that met the criteria at the end of which, you had to start over. I ran everything from 2-day stretches to 11-day stretches, 90%tile, 95%tile, and the all came out looking very similar - hot in the 1930s, cold in the 1960s. We separated the West from the Central-East because they really show a difference. My view is that part of the West's result is UHI and that's a bit of what you are seeing there thanks to the explosive growth around these formerly rural stations. Another interesting finding was the more transient nature of weather in the northern half of CONUS which makes it harder to generate a heatwave of some duration. But in the southern half, things can get stuck.

Hope this helps.

John C.

John R. Christy
Distinguished Professor, Atmospheric and Earth Sciences
Director, Earth System Science Center
Alabama State Climatologist
The University of Alabama in Huntsville
256-961-7763

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John Nielsen-Gammon | Regents Professor and Texas State
Climatologist
Director, Southern Regional Climate Center
Department of Atmospheric Sciences | Texas A&M University
ph: 979-862-2248 | n-g@tamu.edu

artsci.tamu.edu

From: Judith Curry <curry.judith@gmail.com>
Sent: Thursday, August 14, 2025 6:38 PM
To: Steven Koonin
Cc: Roy Spencer; Ross McKittrick; John Christy
Subject: Re: Carbon brief
Attachments: Factcheck: Carbon Brief clea.pdf

Attached is a cleaner version of the msword version that Steve sent.

Solar is another topic that got alot of comments. I suggest i collect the relevant comments and send to soon and Connolly for their input

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I'm (probably) onboard with that as well, but will want to review that entire paragraph.

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Cc: Steven Koonin <steven.koonin@gmail.com>; John Christy <climateman60@gmail.com>
Subject: RE: Carbon brief

I agree.

-Roy

Sent from my Verizon, Samsung Galaxy smartphone

----- Original message -----

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Date: 8/14/25 2:28 PM (GMT-06:00)

To: Ross McKittrick <ross.mckittrick@gmail.com>

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Judith Curry, President

CFAN - Climate Forecast Applications Network

Reno, NV USA

curry.judith@cfanclimate.com | +1.404.803.2012

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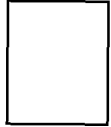
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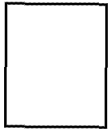
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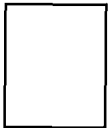
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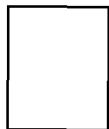
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DOJ 00047360

DOJ_00047363

1. **Definition of global warming**
The term "global warming" refers to the long-term rise in the average temperature of Earth's atmosphere and oceans, which is primarily caused by the increase in greenhouse gas concentrations. This process is driven by human activities, particularly the burning of fossil fuels, which releases large amounts of carbon dioxide and other heat-trapping gases into the atmosphere. These gases trap heat from the sun, leading to a warming effect on the planet's surface and in its atmosphere.

2. **Causes of global warming**
The primary cause of global warming is the increase in greenhouse gas concentrations in the atmosphere. Greenhouse gases, such as carbon dioxide, methane, and nitrous oxide, trap heat from the sun, preventing it from escaping back into space. This process is known as the greenhouse effect. Human activities, particularly the burning of fossil fuels for energy, are the main source of these gases. Other factors contributing to global warming include deforestation, which reduces the number of trees that can absorb carbon dioxide, and land use changes that alter the Earth's albedo (reflectivity).

3. **Effects of global warming**
Global warming has a wide range of effects on the Earth's climate and environment. These include more frequent and severe weather events, such as hurricanes, droughts, and heatwaves. It also leads to the melting of glaciers and ice sheets, which contributes to rising sea levels. Additionally, global warming can disrupt ecosystems, leading to the extinction of species and changes in the timing and intensity of seasonal weather patterns. The overall impact is a significant alteration of the Earth's natural climate system.

4. **Global warming and human health**
Global warming can have direct and indirect effects on human health. Direct effects include increased heat stress, which can lead to heatstroke and other heat-related illnesses. Indirect effects include the spread of vector-borne diseases, such as malaria and dengue fever, as warmer temperatures allow these diseases to spread to new areas. Additionally, global warming can affect food and water security, leading to malnutrition and other health issues. The overall impact on human health is a significant concern, as it can lead to a wide range of health problems and a decrease in the quality of life.

5. **Global warming and the environment**
Global warming has a profound impact on the environment, leading to a wide range of changes in the Earth's climate and ecosystems. These include the melting of glaciers and ice sheets, which contributes to rising sea levels. It also leads to the drying of rivers and lakes, which can lead to water scarcity and drought. Additionally, global warming can lead to the loss of biodiversity, as many species are unable to adapt to the changing climate. The overall impact is a significant alteration of the Earth's natural environment, which can have long-term consequences for the planet's health and the well-being of its inhabitants.

6. **Global warming and the future**
The future of the Earth's climate and environment will depend on the actions we take to address global warming. If we continue to burn fossil fuels at the current rate, the warming will continue to accelerate, leading to even more severe weather events, rising sea levels, and the loss of biodiversity. However, if we take action to reduce greenhouse gas emissions, we can slow down the warming and prevent the most severe consequences of global warming. This requires a combination of individual actions, such as reducing energy consumption and using public transportation, and government policies, such as implementing carbon pricing and investing in renewable energy.

7. **Global warming and the role of science**
Science plays a crucial role in understanding global warming and its effects. Scientists use a variety of methods to study the Earth's climate, including analyzing historical data, conducting experiments, and using computer models. These studies have shown that the warming is real and is caused by human activities. Science also helps us understand the potential impacts of global warming and develop strategies to mitigate its effects. By continuing to advance our understanding of global warming, we can make better decisions about how to address this global challenge.

8. **Global warming and the role of policy**
Policy plays a crucial role in addressing global warming. Governments and international organizations have implemented a variety of policies to reduce greenhouse gas emissions, including carbon pricing, renewable energy incentives, and emissions trading. These policies are essential for slowing down the warming and preventing the most severe consequences of global warming. However, more action is needed, and many countries are still struggling to implement effective policies. The role of policy is to create a framework for action and ensure that all countries are contributing to the effort to address global warming.

9. **Global warming and the role of the public**
The public plays a crucial role in addressing global warming. Individuals can take a variety of actions to reduce their own greenhouse gas emissions, such as using public transportation, conserving energy, and eating a diet that is low in meat. These actions, when taken collectively, can have a significant impact on reducing greenhouse gas emissions. Additionally, the public can influence government policy by voting for candidates who support strong climate action and by contacting their representatives. The role of the public is to create a demand for action and ensure that governments and businesses are held accountable for their actions.

10. **Global warming and the role of the media**
The media plays a crucial role in raising awareness about global warming and its effects. By reporting on the latest scientific findings and the impacts of global warming, the media can help the public understand the urgency of the situation. Additionally, the media can influence public opinion and government policy by highlighting the need for action and by reporting on the actions of governments and businesses. The role of the media is to provide accurate information and to create a sense of urgency about the need to address global warming.

11. **Global warming and the role of the private sector**
The private sector plays a crucial role in addressing global warming. Many companies are investing in renewable energy and other sustainable technologies, which can help reduce greenhouse gas emissions. Additionally, the private sector can influence government policy by lobbying for strong climate action and by implementing sustainable practices within their own organizations. The role of the private sector is to create a market for sustainable products and services and to ensure that all companies are contributing to the effort to address global warming.

12. **Global warming and the role of the international community**
The international community plays a crucial role in addressing global warming. Many countries have signed international agreements, such as the Paris Agreement, which aim to reduce greenhouse gas emissions and prevent the most severe consequences of global warming. These agreements are essential for ensuring that all countries are contributing to the effort to address global warming. However, more action is needed, and many countries are still struggling to implement their commitments. The role of the international community is to create a framework for action and ensure that all countries are contributing to the effort to address global warming.

13. **Global warming and the role of the future generations**
Future generations will be the most affected by global warming. The warming will lead to a wide range of changes in the Earth's climate and environment, which can have long-term consequences for the well-being of future generations. Therefore, it is essential that we take action now to address global warming and prevent the most severe consequences of global warming. The role of future generations is to ensure that the Earth is a healthy and sustainable place for them and for all of us.

14. **Global warming and the role of the present generation**
The present generation has a crucial role in addressing global warming. We are the ones who are causing the warming, and we are the ones who can take action to stop it. By reducing our greenhouse gas emissions and supporting strong climate action, we can slow down the warming and prevent the most severe consequences of global warming. The role of the present generation is to create a better future for ourselves and for future generations.

15. **Global warming and the role of the world**
The world as a whole has a crucial role in addressing global warming. We are all part of the same planet, and we all have a responsibility to take action to address global warming. By working together, we can create a better future for ourselves and for the planet. The role of the world is to create a framework for action and ensure that all countries are contributing to the effort to address global warming.

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DOJ 00047366

Glossary

IPCC: The Intergovernmental Panel on Climate Change, the UN body for assessing the science related to climate change. IPCC works in five-to-eight year cycles and comprises three working groups of scientists, each focusing on different aspects of climate science and the response to climate change. Each assessment cycle, scientists appointed by the IPCC produce three reports summarising the latest climate science, as well as “special reports” and a synthesis report, which serve as key inputs to international climate negotiations.

WG1/2/3: The three working groups of the IPCC. Working Group I (WG1) deals with the physical science basis of climate change, Working Group II (WG2) with climate change impacts, adaptation and vulnerability and Working Group III (WG3) with mitigation of climate change.

AR6: The sixth assessment report of the IPCC, which was published in 2013-14.

AR5: The fifth assessment report of the IPCC, which was published in 2011-12.

CMIP5/6: The Coupled Model Intercomparison Project (CMIP) is the international modelling effort that feeds into the IPCC's influential assessments. CMIP5 was developed for AR5, while **CMIP6** was developed for AR6.

Methodology

The DoE report references around 350 sources. Of these, around 240 are peer-reviewed research papers. Other sources include books, commentary articles and letters published in journals, as well as “grey” literature, such as study preprints, blog posts, news articles and government websites.

For this factcheck, Carbon Brief emailed the corresponding authors of all 240 peer-reviewed studies. If the corresponding author did not respond or the email address bounced, Carbon Brief contacted another report author – typically the lead author, if separate to the corresponding author.

In 20 cases, the corresponding author was also an author on the DoE report itself. These authors were not invited to factcheck their own work.

Carbon Brief asked the corresponding authors to read the section of the report that cites their work and to identify if it uses their work to make “misleading” or “false” statements.

False was described as “statements/statistics that are factually incorrect or untrue”.

Misleading was described as “statements/statistics that are presented in a way that distort the meaning, create a false impression or omit/cherry-pick crucial details”.

(Carbon Brief also invited various topic experts to contribute their responses. Respondents were also invited to forward Carbon Brief's request to other experts.)

Carbon Brief's factcheck also includes comments from scientists who have already reacted to the DoE report in public, via media articles, social media or blog posts. Links to these sources are included with the relevant responses.

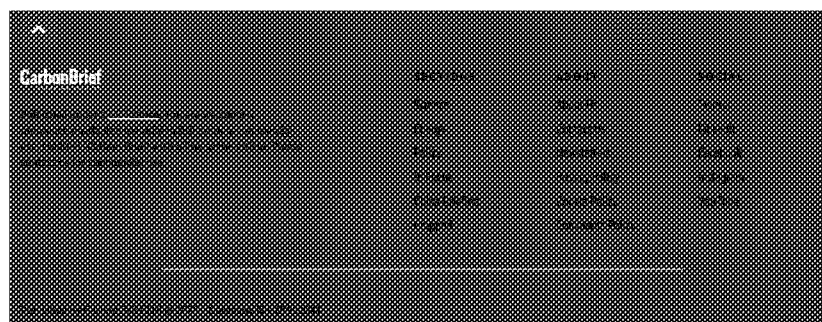
Carbon Brief ensured that any scientists contributing to the factcheck in an anonymous capacity have the appropriate expertise to comment on the sections of the report that they factchecked.

Any pages with “misleading” and “false” statements are shown in orange and red, respectively, in the main graphic. Pages that are not coloured are either accurate, or have not yet been factchecked.

There has been some speculation that artificial-intelligence tools might have been used to generate some parts of the report, particularly the citations, due to some “hallucinatory”-type mistakes. For example, a [Carbon Brief article from 2017](#) is cited, but the credited author is mistakenly listed as another Carbon Brief journalist.

However, Carbon Brief ran samples of the report's main text through several AI text checker tools and found there is a “very low probability” they were generated using AI. It was not possible to do similar checks of the report's citations.

Carbon Brief invites any scientists who notice further errors in the report to email info@carbonbrief.org.



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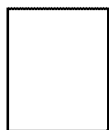
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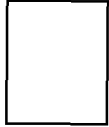
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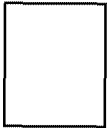
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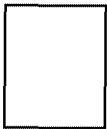
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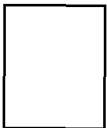
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Subject: RE: Carbon brief

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-Roy

Sent from my Verizon, Samsung Galaxy smartphone

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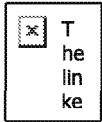
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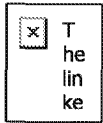
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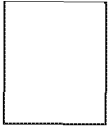
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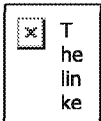
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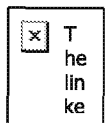
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Attachments: image001.jpg

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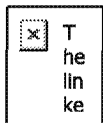
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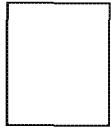
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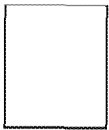
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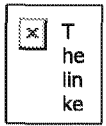


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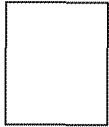
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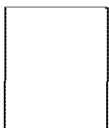
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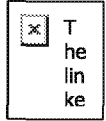


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I assume this is a preview of what we'll face in the public review comments. Some overlap with the internal DoE comments.

If we were asked to respond to Carbon Brief I'd say something like this:

We are going through these comments, which we hope will be submitted to the public docket so that our responses will also be on the public record. In many cases the comment authors ask for more context or acknowledgment of conflicting evidence. There are some specific allegations of inaccuracies or incorrect citations and we will check those especially carefully.

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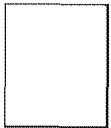
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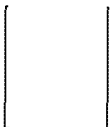
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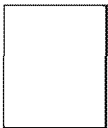
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Cc: Ross McKittrick <ross.mckittrick@gmail.com>; John Christy <climateman60@gmail.com>; Roy W. Spencer <roywspencer@hotmail.com>
Subject: Re: NAS committee membership posted

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<https://www.nationalacademies.org/our-work/anthropogenic-greenhouse-gases-and-us-climate-evidence-and-impacts#sectionCommittee>

Steven Koonin

--



Judith Curry, President
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Network
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curry.judith@cfanclimate.com |
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From: Judith Curry [curry.judith@gmail.com]
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CC: John Christy [climateman60@gmail.com]; Ross McKittrick [ross.mckittrick@gmail.com]; Roy W. Spencer [roywspencer@hotmail.com]
Subject: Re: Carbon brief

THX. I think we should respond to this. Some are substantive, but most can be dealt with as suggested by Steve. I have time to start going through this

On Thu, Aug 14, 2025 at 11:12 AM Steven Koonin <steven.koonin@gmail.com> wrote:
Word doc attached

[I printed the website page from my Chrome browser to a pdf file, then converted the pdf to Word]

SEK

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John C.

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carbon brief fact check is now up

<https://interactive.carbonbrief.org/doe-factcheck/index.html>

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Steven Koonin

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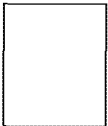
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Subject: RE: Carbon brief
Attachments: image001.jpg; Factcheck_ Trump's climate report includes more than 100 false or misleading claims.docx

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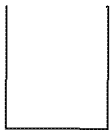
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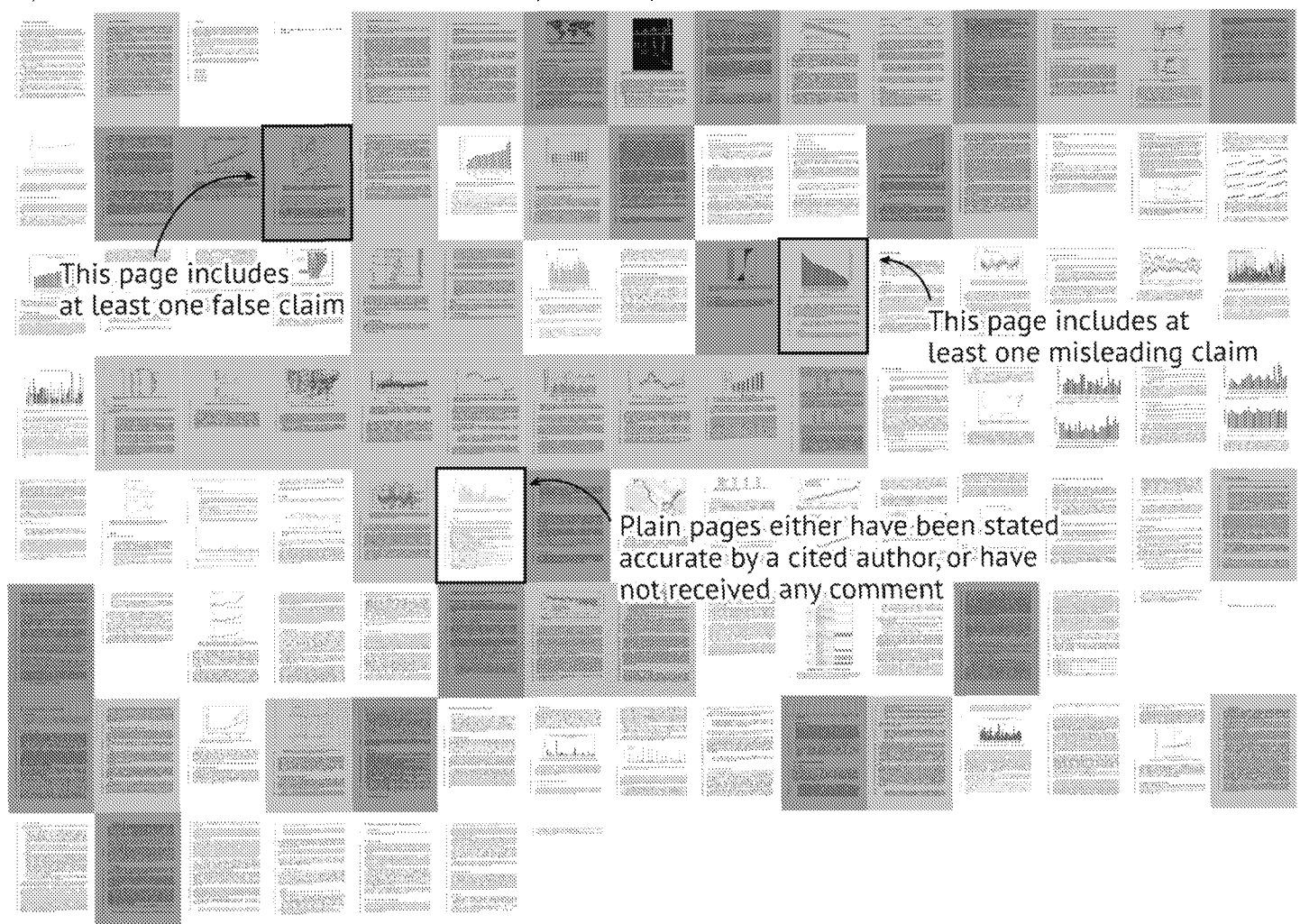


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Factcheck: Trump's climate report includes more than 100 false or misleading claims

By [[HYPERLINK "https://www.carbonbrief.org/author/ayeshatandon/" \h](https://www.carbonbrief.org/author/ayeshatandon/)], [[HYPERLINK "https://www.carbonbrief.org/author/leohickman/" \h](https://www.carbonbrief.org/author/leohickman/)], [[HYPERLINK "https://www.carbonbrief.org/author/cecilia-keating/" \h](https://www.carbonbrief.org/author/cecilia-keating/)] and [[HYPERLINK "https://www.carbonbrief.org/author/robertmcsweeney/" \h](https://www.carbonbrief.org/author/robertmcsweeney/)] Design by [[HYPERLINK "https://www.carbonbrief.org/author/tomprater/" \h](https://www.carbonbrief.org/author/tomprater/)]

13 August 2025



Pages highlighted red contain false statements, whilst pages highlighted orange contain misleading statements. Pages can contain more than one false or misleading statement. Pages that remain uncoloured represent parts of the report that either have been stated as accurate by the cited author, or have not received any comment from invited experts. Front pages, reference and glossary pages omitted.

A “critical assessment” report commissioned by the Trump administration to justify a rollback of US climate regulations contains at least 100 false or misleading statements, according to a Carbon Brief factcheck involving dozens of leading climate scientists.

The 140-page report – “[[HYPERLINK](https://www.energy.gov/sites/default/files/2025-07/DOE_Critical_Review_of_Impacts_of_GHG_Emissions_on_the_US_Climate_July_2025.pdf) "https://www.energy.gov/sites/default/files/2025-07/DOE_Critical_Review_of_Impacts_of_GHG_Emissions_on_the_US_Climate_July_2025.pdf" \h] [[HYPERLINK](#)

"[https://www.energy.gov/sites/default/files/2025-](https://www.energy.gov/sites/default/files/2025-07/DOE_Critical_Review_of_Impacts_of_GHG_Emissions_on_the_US_Climate_July_2025.pdf)

[07/DOE_Critical_Review_of_Impacts_of_GHG_Emissions_on_the_US_Climate_July_2025.pdf](https://www.energy.gov/sites/default/files/2025-07/DOE_Critical_Review_of_Impacts_of_GHG_Emissions_on_the_US_Climate_July_2025.pdf)" – was published by the US Department of Energy (DoE) on 23 July, just days before the government laid out plans to revoke a scientific finding used as the legal basis for emissions regulation.

The executive summary of the controversial report inaccurately claims that “CO2-induced warming might be less damaging economically than commonly believed”.

It also states misleadingly that “excessively aggressive [emissions] mitigation policies could prove more detrimental than beneficial”.

Compiled in just two months by five “independent” researchers hand-selected by the [[HYPERLINK "https://www.bbc.co.uk/news/articles/c93qjdjwnxko" \h](https://www.bbc.co.uk/news/articles/c93qjdjwnxko)] US secretary of energy Chris Wright, the document has [[HYPERLINK "https://www.lemonde.fr/en/environment/article/2025/08/06/scientists-outraged-by-climate-skeptic-report-commissioned-by-trump-administration_6744121_114.html" \h](https://www.lemonde.fr/en/environment/article/2025/08/06/scientists-outraged-by-climate-skeptic-report-commissioned-by-trump-administration_6744121_114.html)] from climate scientists, who have pointed to factual errors, misrepresentation of research, messy citations and the cherry-picking of data.

Experts have also [[HYPERLINK "https://bsky.app/profile/andrewdessler.com/post/3lv5ofrb2e22g" \h](https://bsky.app/profile/andrewdessler.com/post/3lv5ofrb2e22g)] the authors’ track record of promoting views at odds with the mainstream understanding of climate science.

Wright’s department [[HYPERLINK "https://www.energy.gov/articles/department-energy-issues-report-evaluating-impact-greenhouse-gasses-us-climate-invites" \h](https://www.energy.gov/articles/department-energy-issues-report-evaluating-impact-greenhouse-gasses-us-climate-invites)] the report – which is currently open to public comment as part of a 30-day review – underwent an “internal peer-review period amongst [the] DoE’s scientific research community”.

The report is designed to provide a scientific underpinning to one flank of the

Trump administration's plans to [[HYPERLINK](#)

"<https://www.epa.gov/system/files/documents/2025-03/final-pager-endangerment.pdf>" \h] that serves as the legal prerequisite for federal emissions regulation. (The second flank is about [[HYPERLINK](#)

"<https://eelp.law.harvard.edu/epas-proposal-to-eliminate-the-endangerment-finding-and-motor-vehicle-greenhouse-gas-regulations/>" \h] [[HYPERLINK](#)

"<https://eelp.law.harvard.edu/epas-proposal-to-eliminate-the-endangerment-finding-and-motor-vehicle-greenhouse-gas-regulations/>" \h] to regulate emissions.)

The “[[HYPERLINK "https://www.epa.gov/climate-change/endangerment-and-cause-or-contribute-findings-greenhouse-gases-under-section-202a" \h \]”](#) — enacted by the [[HYPERLINK "http://news.bbc.co.uk/1/hi/sci/tech/8004975.stm" \h \]](#) in 2009 — states that six greenhouse gases are contributing to the net-negative impacts of climate change and, thus, put the public in danger.

In a [[HYPERLINK "https://www.epa.gov/newsreleases/epa-releases-proposal-rescind-obama-era-endangerment-finding-regulations-paved-way"](https://www.epa.gov/newsreleases/epa-releases-proposal-rescind-obama-era-endangerment-finding-regulations-paved-way) \h] on 29 July, the US Environmental Protection Agency said “updated studies and information” set out in the new report would “challenge the assumptions” of the 2009 finding.

Carbon Brief asked a wide range of climate scientists, including those cited in the “critical review” itself, to factcheck the report’s various claims and statements.

Flaws visualised

The responses can be explored below, with false statements highlighted in red and misleading statements shaded in orange. Any areas that remain uncoloured represent parts of the report that either have been stated as accurate by a cited author, or have not received any comment from invited experts.

The dropdown menu can be used to navigate to specific sections of the report.

Carbon Brief’s analysis also finds that, of the 350 references included in the report, almost 10% is work by the report’s own authors.

Executive summary

Amid the Trump administration’s [[HYPERLINK "https://www.commondreams.org/news/trump-attack-on-science"](https://www.commondreams.org/news/trump-attack-on-science) \h] on [[HYPERLINK "https://www.science.org/content/article/how-trump-administration-dismantling-science-u-s"](https://www.science.org/content/article/how-trump-administration-dismantling-science-u-s) \h], some contributors have

asked to be anonymised. The responses from scientists have been lightly edited **MISLEADING** for clarity and style.

Elevated concentrations of CO2 directly enhance plant growth, globally contributing to “greening” the planet and increasing agricultural productivity.

Contributors can be found towards the end of the article.

[HYPERLINK "https://profiles.stanford.edu/david-lobell" \h \]](https://profiles.stanford.edu/david-lobell),

associate professor, Stanford University

responded to Carbon Brief's request for comment at the time of going to press.

I see two main problems with this (very old) argument. The direct benefits of CO₂ are widely acknowledged and nothing new. But we know that elevated CO₂ leads to climate changes and so the question is whether the CO₂ benefits are big enough to offset the climate losses. Their report does not address the net effects, which many studies have shown are negative, even for the US. The numbers they cite for direct effects of CO₂ are mainly from co2science.org, which is not a reputable source. Their summaries are not peer reviewed and include many studies of pots in greenhouses which are known to be biased. The numbers cited in the report are more than 2x what the best literature shows, such as in Ainsworth & Long (2021).

Quote from [[HYPERLINK "https://science.feedback.org/review/misleading-u-s-department-energy-climate-report-chooses-bias-over-science-climate-scientists-say/" \h](https://science.feedback.org/review/misleading-u-s-department-energy-climate-report-chooses-bias-over-science-climate-scientists-say/)]

Supporting evidence

[[HYPERLINK "https://onlinelibrary.wiley.com/doi/abs/10.1111/gcb.15375" \h](https://onlinelibrary.wiley.com/doi/abs/10.1111/gcb.15375)]

Part I: Direct human influence on ecosystems and the climate

1 Carbon dioxide as a pollutant

Page 2

MISLEADING

The growing amount of CO₂ in the atmosphere directly influences the Earth system by promoting plant growth (global greening), thereby enhancing agricultural yields, and by neutralising ocean alkalinity.

Anonymous

This ignores other effects of rising CO₂ concentrations, i.e.: on climate. It is also failing to mention that increased CO₂ can reduce the nutrient density of some crops.

Quote given to Carbon Brief

Supporting evidence

[[HYPERLINK "https://www.ipcc.ch/report/ar6/wg2/chapter/chapter-5/" \h](https://www.ipcc.ch/report/ar6/wg2/chapter/chapter-5/)]

2.1 CO₂ as a contributor to global greening

Page 3

FALSE

While plant models predict increased photosynthesis in response to rising CO₂, Haverd et al. (2020) reported a CO₂ fertilisation rate much larger than model predictions. That is, CO₂ fertilisation had driven an increase in observed global photosynthesis by 30% since 1900, versus 17% predicted by plant models. If true it would indicate that global models of the socioeconomic impacts of rising CO₂ have understated the benefits to crops and agriculture.

[[HYPERLINK "https://www.iri-thesys.org/people-pages/delphine-deryng-dr/" \h](https://www.iri-thesys.org/people-pages/delphine-deryng-dr/)], lead author, IPCC AR6 WG2

The paper by Haverd et al. focuses on natural ecosystems, not crops. So whilst the findings that CO₂ fertilisation effects on global greening makes a larger share relative to other factors, the results are not directly transferable to the socio-economic impacts of rising CO₂ on agriculture. Rising CO₂ contributes to

higher radiative forcing which increases global mean temperature and accelerates the global water cycle, causing increases in the severity and frequency of extreme weather events (e.g. droughts, heat-stress and wildfires), particularly threatening crop yields and production.

Quote given to Carbon Brief

Supporting evidence

[[HYPERLINK "https://onlinelibrary.wiley.com/doi/10.1111/gcb.14950" \h](https://onlinelibrary.wiley.com/doi/10.1111/gcb.14950)]

2.1 CO2 as a contributor to global greening

Page 3

MISLEADING

The growing CO2 concentration in the atmosphere has the important positive effect of promoting plant growth by enhancing photosynthesis and improving water use efficiency.

Anonymous

Promoting plant growth is not always positive, because some species benefit more than others, which creates risks to biodiversity. For example, in tropical forests, elevated CO2 promotes the growth of lianas, which are parasites that threaten trees. Also increased CO2 fertilisation is playing a role in disrupting grassland and savannah ecosystems by promoting tree and shrub growth ("woody encroachment").

Quote given to Carbon Brief

Supporting evidence

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2.1 CO2 as a contributor to global greening

Page 3

FALSE

Section 2.1.1

Anonymous

The following section cites papers that are, in fact, showing evidence for other drivers of change, e.g. page 14 line 34 says "Piao et al. (2020) noted that greening was even observable in the Arctic", but Piao et al (2020) actually show that warming is the dominant driver of greening in the Arctic, not CO2 fertilisation (see figure 4 and associated text). Also the authors of the DoE report contradict their own statement two paragraphs later by saying: "Chen et al. (2019) show that in China and India much of it is driven by land management changes."

Quote given to Carbon Brief

Supporting evidence

[[HYPERLINK "https://www.nature.com/articles/s43017-019-0001-x"](https://www.nature.com/articles/s43017-019-0001-x) \I"Sec6" \h]

2 Direct impacts of CO2 on the environment

Page 3

MISLEADING

Piao et al. (2020) and Chen et al. (2024) report that the greening trend continues with no evidence of slowdown.

Anonymous

The DoE authors fail to mention another study which shows the opposite, that greening was reversed around the year 2000 over 90% of the global vegetated area.

Quote given to Carbon Brief

Supporting evidence

[[HYPERLINK "https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2022EF002788"](https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2022EF002788) \h]

2.1 CO2 as a contributor to global greening

Pages 14-17

MISLEADING

Section 2.1

[[HYPERLINK "https://www.agu.org/user-profile?cstkey=E0190A3A-0069-416C-8B0C-4048C7E69CB8"](https://www.agu.org/user-profile?cstkey=E0190A3A-0069-416C-8B0C-4048C7E69CB8) \h], retired atmospheric physicist

Chapters 2 and 9 assert that CO2 fertilisation will increase plant growth and crop yields. The proposed benefits of CO2 fertilisation are not realised in this set of DGVMs because this is only one of several mechanisms that control plant growth. As any farmer knows, plant growth is rarely limited by the abundance of the most abundant nutrient. It is usually limited by the abundance of the

least abundant nutrient. While increased CO₂ can accelerate plant growth in carefully controlled laboratory conditions, this rarely happens in nature or in large-scale agriculture. There, plant growth is usually limited by water, nitrogen, phosphorus, sunlight or temperature. These models include all of those effects. The range of outputs produced by the models reflects uncertainties in the relative roles of these processes and their potential evolution with climate change. This behaviour should foster serious concern (doubt) about the potential benefits of CO₂ fertilisation in a changing climate. There is no discussion of this here or in chapters 2.1 or 9.

Quote given to Carbon Brief

2.1 CO₂ as a contributor to global greening

Page 4

MISLEADING

Had atmospheric CO₂ levels continued declining, plant growth would have declined and eventually ceased. Below 180ppm, the growth rates of many C₃ species are reduced 40-60% relative to 350ppm (Gerhart and Ward (2010)) and growth has stopped altogether under experimental conditions of 60-140ppm CO₂. Some C₄ plants are still able to grow at levels even as low as 10ppm, albeit very slowly (Gerhart and Ward (2010)).

[[HYPERLINK "https://case.edu/provost/about/bio"](https://case.edu/provost/about/bio) \h], provost and executive vice president, Case Western Reserve University

“Ward, however, told WIRED in an emailed statement that her experiments were conducted under ‘highly controlled growth conditions’ to create a ‘mechanistic understanding’ of CO₂, and that climate change can cause a host of impacts on plants not accounted for in her study. ‘With rising CO₂ in natural ecosystems, plants may experience higher heat loads, extreme weather events such as droughts and floods and reduced pollinators – which can have severe net negative effects on plant growth and crop yields,’ she says. ‘Furthermore, our studies indicate that major disruptions in plant development such as flowering time can occur in direct response to rising CO₂, which were not mentioned in the report.’”

Quote from [[HYPERLINK "https://www.wired.com/story/scientists-say-new-government-climate-report-twists-their-work/"](https://www.wired.com/story/scientists-say-new-government-climate-report-twists-their-work/) \h]

2.1 CO₂ as a contributor to global greening

Page 4

FALSE

Piao et al. (2020) and Chen et al. (2024) report that the greening trend continues with no evidence of slowdown, and CO₂ fertilisation remains the dominant driver.

[[HYPERLINK "https://www.iri-thesys.org/people-pages/delphine-deryng-dr/"](https://www.iri-thesys.org/people-pages/delphine-deryng-dr/) \h], lead author, IPCC AR6 WG2

Chen et al (2024) does not back up this statement. In the abstract, the author concluded that greening, whilst still increasing, has slowed down: "Our study highlighted that drought trend did not necessarily trigger vegetation browning,

but slowed down the rate of greening." Piao et al. (2019) looked at the driver of

global greening. They did not look at whether trends in global greening are rising or decreasing. In addition they analysis focused on historical observation (1980-2010) and did not assess future trends.

Quote given to Carbon Brief

Supporting evidence

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2.1 CO2 as a contributor to global greening

Page 4

MISLEADING

Had atmospheric CO2 levels continued declining, plant growth would have declined and eventually ceased. Below 180ppm, the growth rates of many C3 species are reduced 40-60% relative to 350ppm (Gerhart and Ward (2010)) and growth has stopped altogether under experimental conditions of 60-140ppm CO2. Some C4 plants are still able to grow at levels even as low as 10ppm, albeit very slowly (Gerhart and Ward (2010)).

Anonymous

The decline in atmospheric CO2 levels over the last few tens of millions of years stopped naturally, and for the last 800,000 years up until the Industrial Revolution did not show much of a trend, just fluctuating between about 170 and 280 parts per million. The hypothetical scenario of a further decline below these levels is not relevant – it is not the case that human-driven CO2

emissions have somehow saved us from declining CO2 levels and declining plant growth, as seems to be the implication behind this paragraph.

Quote given to Carbon Brief

Supporting evidence

[HYPERLINK "https://www.ipcc.ch/report/ar6/wg1/chapter/chapter-5/" \h]

2.1 CO2 as a contributor to global greening

Page 5

MISLEADING

The overwhelming theme is that plants, especially C3 plants, benefit from extra CO2.

Anonymous

While individual plants benefit in isolation, the overall effect on an ecosystem and biodiversity can be detrimental due to some species benefitting more than others and out-competing them – for example, lianas responding more than trees, which they damage, encroachment of trees and shrubs into grasslands and savannahs, and the promotion of invasive species and weeds.

Quote given to Carbon Brief

Supporting evidence

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2.1 CO2 as a contributor to global greening

Page 5

MISLEADING

There are two mechanisms by which CO2 confers a growth benefit

Anonymous

A "growth benefit" to the plant is not necessarily beneficial in other ways – for example, from IPCC AR6: "Perennial crops and root crops may have a greater capacity for enhanced biomass under elevated CO2 concentrations, although this does not always result in higher yields. For some food crops, nutrient density declines due to elevated CO2." And: "Elevated CO2 reduces some important nutrients such as protein, iron, zinc and some grains, fruit or vegetables to varying degrees depending on crop species and cultivars."

Quote given to Carbon Brief

Supporting evidence

[[HYPERLINK "https://www.ipcc.ch/report/ar6/wg2/chapter/chapter-5/" \h](https://www.ipcc.ch/report/ar6/wg2/chapter/chapter-5/)]

2.1 CO2 as a contributor to global greening

Page 5

MISLEADING

The gains induced by increasing CO2 from 150ppm to 350ppm continue under a further doubling to 700ppm.

Anonymous

This very simplistic illustration from a small laboratory study ignores key effects such as nutrient availability, which in the real world can constrain the response to elevated CO₂.

Quote given to Carbon Brief

Supporting evidence

[[HYPERLINK "https://onlinelibrary.wiley.com/doi/10.1111/gcb.16377"](https://onlinelibrary.wiley.com/doi/10.1111/gcb.16377) \h]

2.2 The alkaline oceans

Pages 17-18

MISLEADING

Section 2.2.1

[[HYPERLINK "https://wmo.int/profile/john-kennedy"](https://wmo.int/profile/john-kennedy) \h], climate scientist

"The first subsection builds towards the conclusion in the summary that 'ocean life is complex and much of it evolved when the oceans were acidic relative to the present'. Leaving aside the vacuity of this argument for the moment – life itself evolved when there was little oxygen in the atmosphere, the biochemical innovations that flooded the atmosphere with oxygen were catastrophic for life then, but see how we would do without it now – their citations are spare and strange particularly within the context of later arguments about the value of models and the contention that this is some sort of meaningful 'critical review'. The first paper they cite regarding long-term change in ocean pH is Krissansen-

Totton et al. (2018), which uses a model to constrain climate and ocean pH of the early Earth up to the present. They find that ocean pH evolves monotonically from 6.6 (see the abstract of the paper for the broad uncertainty ranges) at 4.0 Ga to 7.0 at the Archean-Proterozoic boundary, and to 7.9 at the Proterozoic-Phanerozoic boundary reaching a modern value of 8.2. While we might raise an eyebrow that the 'critical review' finds models are good enough for the herculean task of reproducing almost the whole of Earth's climate history, but not for understanding the past 200 years, the eyebrow is likely to go shooting off your face when you reach the sentence in the 'critical review' that says: 'Even if the water were to turn acidic, it is believed that life in the oceans evolved when the oceans were mildly acidic with pH 6.5 to 7.0.' I derive little comfort from the fact that simple life forms evolved in such conditions. The gist of the 'critical review' isn't that simple life forms will survive the current warming, but that human society supported by a flourishing biosphere will not just survive but thrive. Anyway, this is only part of an argument and the whole of the argument is never really spelled out. It seems to go something like this: pH of the ocean varied in the past and we exist today, therefore we will always exist and pH of the ocean is unimportant.

"The second, shorter, long-term perspective, which feeds into this argument, mixes up surface pH (as shown in Figure 2.3 from the CMEMS dataset) with deep ocean pH (from Rae et al. (2018)) who (according to their abstract) 'present deep-sea coral boron isotope data that track the pH – and thus the CO₂ chemistry – of the deep Southern Ocean over the past forty thousand years'. 'Deep' and 'track' are the operative words here. The estimated changes are 'deep', from a depth of around 750 metres and not the surface. Regarding 'track', the numbers quoted in the 'critical review' – pH of 7.4 to 7.5 20,000 years

ago – came presumably from Figure S1 in the Rae paper, which provides an approximate conversion of the boron isotopes to pH. How very very approximate they are is shown by an inset uncertainty range, which extends from well below 7.4 to well above 7.6 suggesting great care is needed in the interpretation of the absolute pH values."

Quote from [[HYPERLINK "https://diagrammonkey.wordpress.com/2023/04/20/if-a-komodo-dragon-bites-john-then/" \h](https://diagrammonkey.wordpress.com/2023/04/20/if-a-komodo-dragon-bites-john-then/)]

Supporting evidence

[[HYPERLINK "https://www.pnas.org/doi/full/10.1073/pnas.1721296115" \h](https://www.pnas.org/doi/full/10.1073/pnas.1721296115)]

2.2 The alkaline oceans

Pages 18-19

MISLEADING

Section 2.2.2

[[HYPERLINK "https://wmo.int/profile/john-kennedy" \h](https://wmo.int/profile/john-kennedy)], climate scientist

"The first two paragraphs of the 'critical review' are on that famous American landmark the Great Barrier Reef. And why not? Well. One 'why not' can be found in the IPCC report (AR6 WG2). Chapter 11 has a box ('Box 11.2 | The Great Barrier Reef in Crisis'). It doesn't mention ocean acidification as a risk to the GBR at all (though OA is mentioned frequently elsewhere). The two big risks mentioned are bleaching in response to marine heatwaves and erosion caused by tropical cyclones. Neither of these factors is unconnected to climate change. Ocean acidification may not be a risk to the GBR, but climate change certainly is. The AIMS website which is referenced in the 'critical review' even notes 'a

high tolerance in massive Porites to ocean acidification'. The GBR is introduced here as a 2,300km long straw man.

"The rest of the section concerns itself with the more general impacts of ocean acidification. But only glancingly. They cite Browman (2016) on the lack of null results in the literature and offer, as an example, Clements et al. (2021) which is about the direct effects of OA on the behaviour of fish specifically (not the reefs themselves) though it does have a juicy metascientific quote that serves their purpose of suggesting that discussion of the topic is one-sided. If anything, Clements et al. shows that the literature is no longer one-sided so it rather weakens the point they are trying to make...The Browman article is also somewhat meta and points out that papers on ocean acidification were appearing at an average rate of 300 per year between 2006 and 2015, with around 600 articles per year in each of 2013, 2014 and 2015. How many are there now? I don't know. The Browman and Clements articles are both old-as in the context of a fast-moving field. A simple Google Scholar search will show you that not only are there huge numbers of papers mentioning the topic in the past five years, but there are even lots of review papers and meta analyses on the topic which cover a much broader range of impacts. Summarising that literature with just 12 references (including links to the data used) is not adequate by any definition."

Quote from [[HYPERLINK "https://diagrammonkey.wordpress.com/2023/04/20/if-a-komodo-dragon-bites-john-then/"](https://diagrammonkey.wordpress.com/2023/04/20/if-a-komodo-dragon-bites-john-then/) \h]

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2.1 CO2 as a contributor to global greening

Page 6

MISLEADING

Deryng et al. (2016) surveyed evidence on crop water productivity (CWP), the yield per unit of water used, drawing attention to the potential for CO2 both to enhance photosynthesis and to reduce leaf-level transpiration (water loss during leaf respiration). They surveyed all available FACE data (Free Air CO2 Enrichment – see Chapter 9) on crop yield changes for maize (corn), wheat, rice, and soybean and combined it with crop model data simulating yield responses as of 2080 under the extreme RCP8.5 emissions scenario in four growing regions (tropics, arid, temperate and cold) each of which were split into rainfed and irrigated sub-regions. They reported that models without CO2 fertilisation predicted CWP losses in every region, but those were more than offset by CO2 fertilisation so that all regions showed a net CWP gain. Deryng et al. (2016) also reported that negative impacts of warming on wheat and soybean yields were fully offset by CWP gains and mitigated by up to 90% for rice and 60% for maize.

[[HYPERLINK "https://www.iri-thesys.org/people-pages/delphine-deryng-dr/"](https://www.iri-thesys.org/people-pages/delphine-deryng-dr/) \h], lead author, IPCC AR6 WG2

While those are general conclusions of the paper, they are misleading by not mentioning the considerable discrepancy among modelled results. In fact, this paper was the first of its kind to present findings from the first global modelling intercomparison initiative of global gridded crop models, focusing specifically on how state-of-the-art models represented CO2 effects on crop yield and

evapotranspiration, highlighting these effects as a dominant source of uncertainty in the results, outpassing the uncertainty resulting from the use of different climate change projections. The supplementary information of the paper includes the detailed uncertainty analysis. A key message of the paper was also to highlight the needs for further research on the effects of CO2 on crops and their representation in crop models. Toreti et al. (2020) provides a comprehensive review of the uncertainties associated with the effects of elevated CO2 on crops,

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2.1 CO2 as a contributor to global greening

Page 6

FALSE

Deryng et al. (2016) assumed that climate change would “exacerbate water scarcity”. Yet while models do predict that drylands will expand under climate warming, current data show the opposite: greening is happening even in arid areas.

[HYPERLINK "https://www.iri-thesys.org/people-pages/delphine-deryng-dr/"
\h], lead author, IPCC AR6 WG2

Greening and water scarcity are processes related to different carbon and water cycles, respectively. They cannot be directly compared. Deryng et al. (2016)

didn't assume but reported findings from peer-reviewed scientific studies on water scarcity trends. Deryng et al (2016) specifically stated: "Research indicates unabated climate change will exacerbate water scarcity around the world. This is thought to threaten agricultural productivity and food security, especially in arid regions, where agriculture relies heavily on irrigation and consumes the majority of diverted freshwater."

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2.1 CO2 as a contributor to global greening

Page 6

FALSE

The IPCC has only minimally discussed global greening and CO2 fertilisation of agricultural crops. The topic is briefly acknowledged in a few places in the body of the IPCC 6th and earlier assessment reports but is omitted in all summary documents. Section 2.3.4.3.3 of the AR6 WG1 report, entitled “global greening and browning,” points out that the IPCC special report on climate change and land had concluded with high confidence that greening had increased globally over the past two-to-three decades. It then discusses that there are variations in the greening trend among data sets, concluding that while they have high confidence greening has occurred, they have low confidence in the magnitude of the trend. There are also brief mentions of CO2

fertilization effects and improvements in water use efficiency in a few other chapters in the AR6 WG1 and 2 reports.

[[HYPERLINK "https://www.iri-thesys.org/people-pages/delphine-deryng-dr/"](https://www.iri-thesys.org/people-pages/delphine-deryng-dr/) \h], lead author, IPCC AR6 WG2

Chapter 5 of IPCC WG2 AR6 has reported the state of knowledge of CO₂ fertilisation effects on crops, including photosynthesis stimulation, evapotranspiration reductions, as well as elevated CO₂ effects on the quality of crops. These are widely discussed throughout the chapter, including in figures 5.6, 5.7, and 5.11, as well as in sections 5.2.1 (Detection and Attribution of Observed Impacts), 5.4.1 (Observed Impacts of crop-based systems), 5.4.3 (Projected Impacts of crop-based systems), 5.4.4 (Adaptation Options of crop-based systems), 5.5.3 (Projected Impacts of Livestock-Based Systems), 5.6.2 (Projected Impacts of forestry systems), 5.10.3 (Projected Impacts of mixed systems), 5.12.2 (Mechanisms for Climate Change Impacts on Food Security), 5.12.4 (Projected Impacts on Food Security). The effects of elevated CO₂ on food systems are also included in the summary tables 5.9 (Observed and predicted impacts of climate change on selected medicinal plant species) and 5.14 (Impacts from climate change drivers on the four dimensions of food security).

Quote given to Carbon Brief

Supporting evidence

[[HYPERLINK "https://www.ipcc.ch/report/ar6/wg2/chapter/chapter-5/"](https://www.ipcc.ch/report/ar6/wg2/chapter/chapter-5/) \h]

2.1 CO₂ as a contributor to global greening

FALSE

Overall, however, the policymaker summaries, technical summaries and synthesis reports of AR5 and AR6 do not discuss the topic.

[[HYPERLINK "https://www.iri-thesys.org/people-pages/delphine-deryng-dr/"](https://www.iri-thesys.org/people-pages/delphine-deryng-dr/) \h], lead author, IPCC AR6 WG2

The technical summary does include a statement of CO2 fertilisation effects on vegetation and crops. Specifically in the observed and projected impacts sections (TS.B.1.5 and TS.C.1.4) as well as in Figure TS.6 FOOD-WATER. The summary for policymakers includes a summary of the findings. In the case of agriculture the overall key risks are reported, which are based on the overall assessment of observed and projected impacts of climate change, including effects of CO2 fertilisation, as well as potential contribution of adaptation measures on global and regional food production and food security.

Quote given to Carbon Brief

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2.1 CO2 as a contributor to global greening

Page 6

MISLEADING

The IPCC has only minimally discussed global greening and CO2 fertilisation of agricultural crops.

Anonymous

The IPCC has extensively discussed the promotion of plant growth by rising atmospheric CO₂ concentrations and consequent effects on global vegetation and land carbon sinks, including in summary documents – just not using the specific term "global greening" since other terms are used. The AR6 WG1 carbon cycle chapter prominently discusses the uptake of carbon in land ecosystems, e.g. in the executive summary page ("The ocean and land sinks of CO₂ have continued to grow over the past six decades in response to increasing anthropogenic CO₂ emissions (high confidence)") and in Frequently Asked Question 5.1. The WG1 summary for policymakers includes a figure showing the land carbon sink (Figure SPM.7). In WG2, chapter 2 on terrestrial ecosystems also discusses this, e.g. in the executive summary: "Biome shifts and structural changes within ecosystems have been detected at an increasing number of locations, consistent with climate change and increasing atmospheric CO₂ (high confidence)" and "A combination of changes in grazing, browsing, fire, climate and atmospheric CO₂ is leading to observed woody encroachment into grasslands and savannah."

The WG2 technical summary includes a figure showing woody encroachment (Figure TS12) and the WG2 summary for policymakers states that "at the global scale, terrestrial ecosystems currently remove more carbon from the atmosphere than they emit", but also notes: "However, recent climate change has shifted some systems in some regions from being net carbon sinks to net carbon sources." (page 20 footnote 39). The WG2 food chapter extensively discusses effects of elevated CO₂ on crops, including both past and projected future changes. The summary documents focus on overall outcomes from the

combined effects of climate change, elevated CO2 and other changes in atmospheric composition. The IPCC also specifically notes that elevated CO2 can reduce nutritional quality in crops.

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2.1 CO2 as a contributor to global greening

Page 6

FALSE

The topic is briefly acknowledged in a few places in the body of the IPCC 6th and earlier assessment reports.

Anonymous

The topic of CO2 fertilisation effects, including their limitations and interactions with other drivers of change, are extensively discussed in AR6 WG2 chapter 5 (Food, Fibre and Other Ecosystem Products). The term "CO2" appears 136 times in that chapter. In the WG1 report, Chapter 5 is "Global Carbon and Other Biogeochemical Cycles and Feedbacks" and, again, extensively includes discussion of CO2 effects as part of the carbon cycle.

Quote given to Carbon Brief

Supporting evidence

[HYPERLINK "https://www.ipcc.ch/report/ar6/wg2/chapter/chapter-5/" \h]

2.1 CO2 as a contributor to global greening

Page 6

FALSE

Overall, however, the policymaker summaries, technical summaries and synthesis reports of AR5 and AR6 do not discuss the topic.

Anonymous

The WG1 summary for policymakers includes a figure showing the land carbon sink (Figure SPM.7) and the caption includes: "Land and ocean carbon sinks respond to past, current and future emissions...During the historical period (1850-2019) the observed land and ocean sink took up 1430 GtCO₂ (59% of the emissions)." The WG2 technical summary includes a figure showing woody encroachment (Figure TS12) and the WG2 summary for policymakers states: "At the global scale, terrestrial ecosystems currently remove more carbon from the atmosphere than they emit." It also notes: "However, recent climate change has shifted some systems in some regions from being net carbon sinks to net carbon sources." In AR5, the WG1 SPM says: "Of these cumulative anthropogenic CO₂ emissions...160 [70 to 250] GtC have accumulated in natural terrestrial ecosystems (i.e., the cumulative residual land sink)."

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2.2 The alkaline oceans

Page 7

MISLEADING

Even if the water were to turn acidic, it is believed that life in the oceans evolved when the oceans were mildly acidic with pH 6.5 to 7.0 (Krissansen-Totton et al. (2018)).

[[HYPERLINK](#)

"<https://depts.washington.edu/astrobio/wordpress/profile/joshua-krissansen-totton/>" \h], assistant professor, Earth and space sciences , University of Washington

“Krissansen-Totton told WIRED in an email that his work on ocean acidity billions of years ago has ‘no relevance’ to the impacts of human-driven ocean acidification today and that today calcium carbonate saturation is quickly diminishing in the ocean alongside rising acidity. Dissolved calcium carbonate is essential for many marine species, particularly those that rely on it to build their shells.’The much more gradual changes in ocean pH we observe on geologic timescales were typically not accompanied by the rapid changes in carbonate saturation that human CO₂ emissions are causing, and so the former are not useful analogs for assessing the impact of ocean acidification on the modern marine biosphere,’ he says.”

Quote from [[HYPERLINK "https://www.wired.com/story/scientists-say-new-government-climate-report-twists-their-work/"](https://www.wired.com/story/scientists-say-new-government-climate-report-twists-their-work/) \h]

2.2 The alkaline oceans

Page 7

MISLEADING

Even if the water were to turn acidic, it is believed that life in the oceans evolved when the oceans were mildly acidic with pH 6.5 to 7.0 (Krissansen-Totton et al. (2018).

[[HYPERLINK "https://www.st-andrews.ac.uk/earth-sciences/people/jwbr/" \h](https://www.st-andrews.ac.uk/earth-sciences/people/jwbr/)], reader, geochemistry and climate, University of St Andrews

Modern marine life and ecosystems (e.g. coral reefs and open ocean fisheries) are very different from the initial lifeforms that evolved in the earliest oceans (e.g. bacteria, single-celled organisms) and so have different adaptive ranges and vulnerabilities. In the same way that the environmental conditions which were suitable for dinosaurs have little relevance for optimal conditions for humans, there is little relevance in comparing early ocean bacterial habitats to the conditions to which modern marine life has adapted.

Quote given to Carbon Brief

2.2 The alkaline oceans

Page 7

MISLEADING

On the time scale of thousands of years, boron isotope proxy measurements show that ocean pH was around 7.4 or 7.5 during the last glaciation (up to

about 20,000 years ago) increasing to present-day values as the world warmed during deglaciation (Rae et al. (2018)).

[[HYPERLINK "https://www.st-andrews.ac.uk/earth-sciences/people/jwbr/" \h](https://www.st-andrews.ac.uk/earth-sciences/people/jwbr/)],
reader, geochemistry and climate, University of St Andrews

The use of "ocean pH" in this statement is misleading, as it seems to imply whole ocean pH or (following from the text above) the pH of the surface ocean, whereas the boron isotope study referenced here (of which I was the first author) apply only to the deep Southern Ocean. These are completely different habitats occupied by completely different species. The habitats of most concern with respect to ocean acidification are in the surface (e.g. coral reefs, key fisheries), where pH was higher than today during the last ice age (e.g. Shao et al., 2019), not lower as suggested in figure 5 of Shao et al. (2019).

Quote given to Carbon Brief

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2.2 The alkaline oceans

Page 7

MISLEADING

Thus, ocean biota appear to be resilient to natural long-term changes in ocean pH since marine organisms were exposed to wide ranges in pH.

[[HYPERLINK "https://www.st-andrews.ac.uk/earth-sciences/people/jwbr/" \h](https://www.st-andrews.ac.uk/earth-sciences/people/jwbr/)],
reader, geochemistry and climate, University of St Andrews

Natural long-term changes in ocean pH have a fundamentally different impact on ocean chemistry than rapid input of carbon from fossil fuels (see Honisch et al. (2011)). When ocean pH is lowered rapidly (e.g. on decadal to centennial timescales), the abundance of carbonate ions decreases rapidly. As these carbonate ions are the most critical ingredient for marine shell formation, shells become harder to grow. Technically, this is known as a drop in calcium carbonate saturation state and it makes calcium carbonate shell formation more energetically costly (e.g. Gagnon et al., (2021)). In contrast, when ocean pH is lowered slowly (e.g. on timescales of a thousand years or more), the dissolution of seafloor calcium carbonate, alongside weathering of rocks on land, has a buffering effect on ocean saturation state, allowing shell formation to continue unimpeded despite the lower pH. Today, anthropogenic CO₂ emission is lowering ocean pH on decadal to centennial timescales, which are too rapid for these buffering processes to keep up with, resulting in lower calcium carbonate saturation states, and making shell formation more energetically costly and enhancing environmental stress on these organisms.

Quote given to Carbon Brief

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2.2 The alkaline oceans

Page 8

MISLEADING

Similarly, a meta-analysis (Clements et al. (2021)) of the negative effects of ocean acidification on reef fish behavior found what they called a “decline effect”: initially dramatic conclusions published in prominent journals showing apparently large impacts of acidification tended to be followed up by subsequent studies on larger sample sizes yielding much smaller and typically non-existent effects. They call for their colleagues to improve research practices to counter the “decline effect”: [The] vast majority of studies with large effect sizes in this field tend to be characterised by low sample sizes, yet are published in high-impact journals and have a disproportionate influence on the field in terms of citations. We contend that ocean acidification has a negligible direct impact on fish behaviour, and we advocate for improved approaches to minimise the potential for a decline effect in future avenues of research (Clements et al. (2021)).

[[HYPERLINK "https://jclementsphd.ca/"](https://jclementsphd.ca/) \h], marine ecologist, Fisheries and Oceans Canada (Gulf Region)

“Clements...says that the way the DoE report cites his research on ocean acidification and fish behavior is accurate ‘from an explicit textual perspective’...Clements says in an email to WIRED that just because his review of the literature found fish behavior to be relatively unaffected by ocean acidification does not mean that a myriad of other ocean ecosystems, biological processes and species will fare similarly. Other work from his lab, meanwhile, has underscored the vulnerability of mussels to ocean warming and looked at how heat waves negatively alter clam behavior...’I want to make it clear that our results should not be interpreted to mean ocean acidification (or climate change more generally) is not a problem,’ he tells WIRED. ‘While effects on fish behavior may not be as severe as initially thought, other species and biological

processes are certainly vulnerable to the impacts of acidification and the compendium of other climate change stressors that our oceans are experiencing.’”

Quote from [[HYPERLINK "https://www.wired.com/story/scientists-say-new-government-climate-report-twists-their-work/" \h](https://www.wired.com/story/scientists-say-new-government-climate-report-twists-their-work/)]

2.2 The alkaline oceans

Page 8

MISLEADING

Similarly, a meta-analysis (Clements et al. (2021)) of the negative effects of ocean acidification on reef fish behaviour found what they called a “decline effect”: initially dramatic conclusions published in prominent journals showing apparently large impacts of acidification tended to be followed up by subsequent studies on larger sample sizes yielding much smaller and typically non-existent effects.

[[HYPERLINK "https://www.ntnu.edu/employees/fredrik.jutfelt" \h](https://www.ntnu.edu/employees/fredrik.jutfelt)], professor of fish ecophysiology, Norwegian University of Science and Technology

The way the DoE report cites our own work is flawed, as they use a specific finding in a small subsection of the field to represent the state of the entire field. This is intellectually dishonest and unscientific, and suggests either gross incompetence or an underlying bias.

Quote given to Carbon Brief

Supporting evidence

[[HYPERLINK "https://journals.plos.org/plosbiology/article?id=10.1371/journal.pbio.3001511" \h](https://journals.plos.org/plosbiology/article?id=10.1371/journal.pbio.3001511)]

2.2 The alkaline oceans

Page 8

MISLEADING

In summary, ocean life is complex and much of it evolved when the oceans were acidic relative to the present. The ancestors of modern coral first appeared about 245m years ago. CO2 levels for more than 200m years afterward were many times higher than they are today. Much of the public discussion of the effects of ocean “acidification” on marine biota has been one-sided and exaggerated.

[[HYPERLINK "https://wmo.int/profile/john-kennedy" \h](https://wmo.int/profile/john-kennedy)], climate scientist

The factoid about coral appearing 245m years ago is unreferenced and not previously mentioned in the section. The contention regarding ‘much of the public discussion’ is likewise unreferenced and not mentioned elsewhere in the section. Public discussion and scientific discussion are two different things. Public discussion isn’t discussed at all and, anyway, the ‘critical review’ is supposedly reviewing the science. I think it’s safe to say it has failed there too.

Quote from [[HYPERLINK](https://diagrammonkey.wordpress.com/2023/04/20/if-a-komodo-dragon-bites-john-then/)

"https://diagrammonkey.wordpress.com/2023/04/20/if-a-komodo-dragon-bites-john-then/" \h]

2.2 The alkaline oceans

Page 8

MISLEADING

In summary, ocean life is complex and much of it evolved when the oceans were acidic relative to the present. The ancestors of modern coral first appeared about 245m years ago. CO2 levels for more than 200m years afterward were many times higher than they are today. Much of the public discussion of the effects of ocean "acidification" on marine biota has been one-sided and exaggerated.

[[HYPERLINK "https://www.st-andrews.ac.uk/earth-sciences/people/jwbr/" \h](https://www.st-andrews.ac.uk/earth-sciences/people/jwbr/)], reader, geochemistry and climate, University of St Andrews

As discussed above, long-term geological pH changes are not a good analogue for a rapid drop in pH of the type happening today, for the key reason that a rapid pH drop results in lowered calcium carbonate saturation state, which makes shell formation harder (e.g. Honisch et al., (2011)). To find better analogues for modern pH decline in the geological record, we need to look at times when ocean pH dropped more quickly. Data of this type are compiled in a recent study by Trudgill et al. (2025). These show that all of the major mass extinction events in the ocean for which we have pH reconstructions are associated with a rapid drop in ocean pH. The geological record thus suggests that rapid pH decline is a matter for serious concern for marine ecosystems and the communities and economies which they support.

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Chen, X., Wang, Y., Liu, Y., and Piao, S. (2024). The global greening continues despite increased drought stress since 2000. Global Ecology and Conservation, 49, e02791.

Anonymous

The author list is incorrect on this citation. It should be Xin Chen, Tiexi Chen, Bin He, Shuci Liu, Shengjie Zhou and Tingting Shi. It falsely associates the paper with well-known authors and demonstrates lack of attention to detail.

Quote given to Carbon Brief

Supporting evidence

[[HYPERLINK "https://www.sciencedirect.com/science/article/pii/S2351989423004262"](https://www.sciencedirect.com/science/article/pii/S2351989423004262) \h]

3 Human influences on the climate

Page 11

MISLEADING

The IPCC has downplayed the role of the sun in climate change, but there are plausible solar irradiance reconstructions that imply it contributed to recent warming.

[[HYPERLINK "https://www.mps.mpg.de/solar-physics/staff/27569"](https://www.mps.mpg.de/solar-physics/staff/27569) \h],

astrophysicist, Max Planck Institute for Solar System Research

This is a vague and potentially misleading statement. The IPCC AR6 adopted the solar radiative forcing values recommended by CMIP6, while AR5 used those from CMIP5. In both cases, the TSI [total solar irradiance] reconstructions selected were the most advanced and extensively scrutinised available at the time (Matthes et al. (2017)). The notion that there exist "plausible" solar irradiance reconstructions implying a solar contribution to recent warming is misleading, since it leaves it vague as to how much it contributed. The most scientifically robust irradiance reconstructions indicate that this contribution has been relatively small in recent decades. The wording of this statement appears to implicitly reference the work of Connolly et al. (2021) and (2023), although only the former is cited in the DoE report. Connolly et al. (2021) argued that a significant portion, or even most, of recent global warming could be attributed to solar variability. However, their approach involved cherry-picking and balancing 16 TSI reconstructions: eight with low long-term variability and eight with high variability. To create this balance, they included outdated or superseded models while omitting many low-variability reconstructions.

The high-variability series used by Connolly et al. (2021):

- Hoyt and Schatten (1993) has been thoroughly discredited (see Chatzistergos (2024)). It includes arbitrary manual adjustments and even fabricated data, with some values seemingly copied from another solar index.
- Lean et al. (1995) is an early version of the NRL [Naval Research Laboratory] model that has since been superseded by multiple updated versions, most recently NRLTSI2/NNL (Coddington et al. (2016) and (2019)).
- Bard et al. (2000) used a simplified linear conversion from cosmogenic isotope production (e.g., ^{10}Be) to TSI, without accounting for nonlinear effects such as

geomagnetic modulation, atmospheric transport processes, or climate-driven deposition variations. These omissions have been addressed in more physically realistic models like SATIRE-M [Spectral And Total Irradiance REconstructions] (Wu et al. (2018)), which show significantly lower long-term variability.

Furthermore, Delaygue and Bard (2011) revised the original Bard et al. (2000) reconstruction which was exhibiting low long-term variability, yet this update was ignored by Connolly et al. (2021).

- Shapiro et al. (2011), an older high-variability model, was effectively replaced by Egorova et al. (2018), which itself has been criticised by Yeo et al. (2020) for exaggerating long-term TSI changes. - Egorova et al. (2018) was included four times in the Connolly et al. (2021) analysis. Including Shapiro et al. (2011), a predecessor of Egorova's model, the same model family was effectively used five times, artificially inflating the weight of high-variability reconstructions.

Crucially, in all these high-variability models, the long-term TSI trends were not derived from observational constraints but were imposed as assumptions. Modern revisions of the NRL model (Lean (2018) and Coddington et al. (2019)) and the Hoyt and Schatten model (Chatzistergos (2024)) have significantly reduced these assumed trends to align with observational evidence. Therefore, treating these outdated, superseded, or methodologically flawed reconstructions as equally probable alternatives to modern, validated TSI models is scientifically unjustified. It creates a false equivalence that misrepresents the current state of solar irradiance research. Connolly et al. (2023), although not cited in the DoE report, extended the analysis to 27 TSI series. However, the same methodological issues persist. Many of the added reconstructions are either outdated or redundant versions of older models. Notably, the highest solar attribution results again stem from Hoyt and

Schatten (1993) and Bard et al. (2000), both of which have been superseded and shown to be unreliable. Finally, the attribution method used by Connolly et al. (2021) was directly critiqued by Richardson and Benestad (2022), who identified substantial flaws in their regression methodology and treatment of uncertainty. Even with the analysis presented in Connolly et al. (2021) and (2023) when the outdated, superseded and implausible TSI series are removed, the result is that the solar contribution to global warming is significantly smaller than the anthropogenic one.

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3.1 Components of radiative forcing and their history

Page 12

MISLEADING

The IPCC assesses the change in the radiative forcing by the sun to be negligible, based on their preference for data reconstructions that imply minimal solar change since pre-industrial times.

[[HYPERLINK "https://www.mps.mpg.de/solar-physics/staff/27568"](https://www.mps.mpg.de/solar-physics/staff/27568) \h],
astrophysicist, Max Planck Institute for Solar System Research

The selection of TSI reconstructions for use in CMIP6 and IPCC AR6 was based on the scientific robustness and performance of the models, rather than the magnitude of TSI variability they imply (Matthes et al. (2017)). The two reconstructions adopted were SATIRE and NRLTSI (now referred to as NNLTSI). These models were chosen because they represent the most advanced TSI reconstructions currently available. SATIRE is a semi-empirical, physics-based model, while NNLTSI is a proxy-based regression model. Both have been extensively validated and demonstrate excellent agreement with direct satellite measurements of TSI, making them the most reliable choices for climate modeling to date.

Quote given to Carbon Brief

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[[HYPERLINK "https://gmd.copernicus.org/articles/10/2247/2017/gmd-10-2247-2017-discussion.html"](https://gmd.copernicus.org/articles/10/2247/2017/gmd-10-2247-2017-discussion.html) \h]

3.1 Components of radiative forcing and their history

Page 12

MISLEADING

But Connolly et al. (2021) reviewed 16 different Total Solar Irradiance (TSI) reconstructions in the literature covering the years 1600-2000; the reconstructions vary from almost no change in TSI to a relatively large upward trend. Those authors note that the variation in TSI reconstructions combined with variations in surface temperature reconstructions allows for inferences consistent with either no or most 20th century warming being attributable to the sun.

[[HYPERLINK "https://www.mps.mpg.de/solar-physics/staff/27564"](https://www.mps.mpg.de/solar-physics/staff/27564) \h],

astrophysicist, Max Planck Institute for Solar System Research

The approach by Connolly et al. (2021) involved cherry-picking and balancing 16 TSI reconstructions: eight with low long-term variability and eight with high variability. To create this balance, they included outdated or superseded models while omitting many low-variability reconstructions.

The high-variability series used by Connolly et al. (2021):

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- Bard et al. (2000) used a simplified linear conversion from cosmogenic isotope production (e.g., ^{10}Be) to TSI, without accounting for nonlinear effects such as geomagnetic modulation, atmospheric transport processes, or climate-driven deposition variations. These omissions have been addressed in more physically realistic models like SATIRE-M [Spectral And Total Irradiance REconstructions] (Wu et al. (2018)), which show significantly lower long-term variability.

Furthermore, Delaygue and Bard (2011) revised the original Bard et al. (2000) reconstruction which was exhibiting low long-term variability, yet this update was ignored by Connolly et al. (2021).

- Shapiro et al. (2011), an older high-variability model, was effectively replaced by Egorova et al. (2018), which itself has been criticised by Yeo et al. (2020) for exaggerating long-term TSI changes.
- Egorova et al. (2018) was included four

times in the Connolly et al. (2021) analysis. Including Shapiro et al. (2011), a predecessor of Egorova's model, the same model family was effectively used five times, artificially inflating the weight of high-variability reconstructions.

Crucially, in all these high-variability models, the long-term TSI trends were not derived from observational constraints but were imposed as assumptions. Modern revisions of the NRL model (Lean (2018) and Coddington et al. (2019)) and the Hoyt and Schatten model (Chatzistergos (2024)) have significantly reduced these assumed trends to align with observational evidence. Therefore, treating these outdated, superseded, or methodologically flawed reconstructions as equally probable alternatives to modern, validated TSI models is scientifically unjustified. It creates a false equivalence that misrepresents the current state of solar irradiance research.

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3.1 Components of radiative forcing and their history

Page 13

MISLEADING

A particularly thorny issue is the gap in TSI data between 1989 and 1991 due to a delay in the launch of a monitor following the Space Shuttle Challenger

disaster on 28 January 1986.

[[HYPERLINK "https://www.mps.mpg.de/solar-physics/staff/27563"](https://www.mps.mpg.de/solar-physics/staff/27563) \h],
astrophysicist, Max Planck Institute for Solar System Research

It is an overstatement to characterise the “ACRIM-gap” [A gap in TSI measurements between the ACRIM-1 and ACRIM-2 satellite monitoring experiments] as a "particularly thorny" issue. While it does contribute to uncertainty in the long-term trend of direct TSI measurements, a growing body of evidence challenges the notion of a TSI increase during the ACRIM-gap, evidence which the DoE report entirely overlooks. Notable examples include Amdur & Huybers (2023), Chatzistergos et al. (2025) and Krivova et al. (2009).

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3.1 Components of radiative forcing and their history

Page 13

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Connolly et al. (2021) found that the IPCC's consensus statements on solar forcing were formulated prematurely through the suppression of dissenting scientific opinions.

[[HYPERLINK "https://www.mps.mpg.de/solar-physics/staff/27559" \h](https://www.mps.mpg.de/solar-physics/staff/27559)],
astrophysicist, Max Planck Institute for Solar System Research

This is yet another statement in the DoE report that is vague. It is not clear what they refer to with consensus, is this about the long-term reconstructions, is it just about the ACRIM-gap that this paragraph discusses? In any case, it appears as a general statement which as such has to be labelled as false. The claim that dissenting scientific opinions are being suppressed is unfounded. Scientific consensus evolves through rigorous evaluation, not censorship. When significant methodological issues are identified, such as the arbitrary adjustments and fabricated data in the Hoyt and Schatten (1993) TSI reconstruction (Chatzistergos (2024)), it is scientifically appropriate to disregard such models as implausible. This is not suppression of dissenting voices, but the enforcement of standards for methodological soundness.

Another example is the Bard et al. (2000) TSI reconstruction, which has since been superseded by more advanced models. Bard et al. (2000) relied on a simplistic linear scaling of cosmogenic isotope production to estimate TSI variability, without accounting for the non-linear influence of geomagnetic field modulation on isotope production. This omission leads to inaccurate estimates of solar variability. Later reconstructions, such as those based on the SATIRE-M framework (e.g., Wu et al. (2018)), incorporate these critical non-linearities and have been shown to produce more consistent results with observational constraints. Albeit, uncertainties remain and more work is being invested in improving these reconstructions too. Furthermore, we have advanced physics-based irradiance reconstruction models, e.g. SATIRE-3D (Yeo et al. (2017) and (2020)), which have been used to set robust constraints on the

magnitude of plausible TSI variations. As a result, irradiance reconstructions that exceed this constraint are considered less plausible than those that remain within it. Disregarding outdated or methodologically flawed reconstructions is a hallmark of scientific integrity, not suppression. The continued use of discredited models undermines robust scientific discourse and misrepresents the state of knowledge. A reminder here that the study by Connolly et al. (2021) cherry-picked 16 TSI reconstructions, deliberately balancing the sample by including eight with small long-term trends and eight with pronounced trends. The high-variation models they used are Hoyt and Schatten (1993), Lean (1995), Bard et al. (2000), Shapiro et al. (2011) and Egorova et al. (2018; used 4 different versions). Besides the issues with Hoyt and Schatten (1993) and Bard et al. (2000) discussed above, Lean (1995) is an outdated precursor to the NRL (now NNL) model and is no longer considered plausible. Shapiro et al. (2011) and Egorova et al. (2018) are different versions of the same model both exceeding the Yeo et al. 2020 constraint.

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3.1 Components of radiative forcing and their history

Pages 22-24

MISLEADING

Section 3.1.1

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The paper by Jenkins et al. (2023) did not include volcanic aerosol effects and thus estimated the wrong sign of the Hunga volcano radiative forcing. Subsequent papers (including my own) agree that Hunga would produce slight cooling. The inclusion of the Jenkins paper here suggests more uncertainty in the calculation than exists. All models and data analyses agree that Jenkins was incorrect (e.g. Stenchikov et al. (2025) and Zhuo et al. (2025)).

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3.2 Future emission scenarios and the carbon cycle

Page 15

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Comparisons of past scenario groups against observations show that IPCC emission projections have tended to overstate actual subsequent emissions. For the IPCC third and fourth Assessment Reports a set of emission projections from the special report on emission scenarios was used; these were referred to as the SRES scenarios. McKittrick et al. (2012) showed that, when converted to per capita values, the SRES scenario emissions distribution was skewed upwards compared with observed trends.

[[HYPERLINK "https://berkeleyearth.org/team/"](https://berkeleyearth.org/team/) \l "zeke-hausfather" \h],
research scientist, Berkeley Earth

While some past IPCC emissions scenarios have overstated future total CO2 emissions (IS92B, A1F1, RCP8.5), others have understated them (IS92D, B2, RCP6). In general total CO2 emissions were on the high end of the range of both the 1992 IS92 scenarios and the 2000 SRES scenarios through 2015, before falling closer to the middle of the range in recent years. Notably both IS92 and SRES scenarios were all variations of baseline scenarios that did not explicitly include aggressive emissions mitigation.

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3.2 Future emission scenarios and the carbon cycle

Page 15

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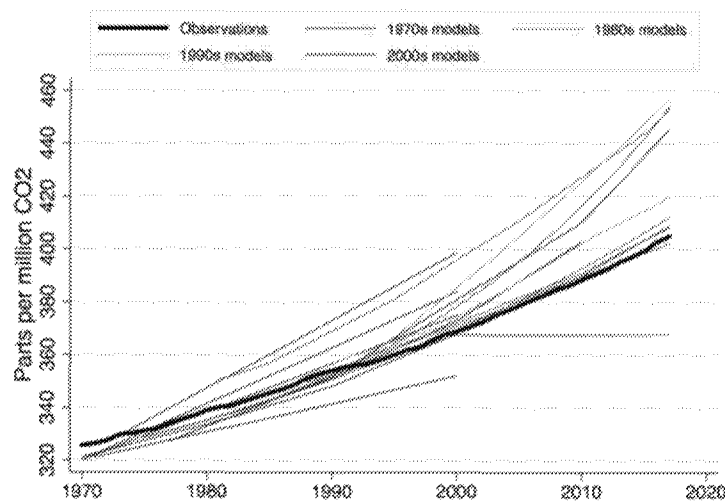


Figure 3.2.1 Since the 1970s successive families of emission and concentration projections (colored lines) have consistently overestimated observations (black line). Source: Hausfather *et al.* (2019) Figure S4.

[[HYPERLINK "https://berkeleyearth.org/team/"](https://berkeleyearth.org/team/) \l "zeke-hausfather" \h],
research scientist, Berkeley Earth

Figure 3.2.1 focuses primarily on older climate models that predate the SRES scenarios. The only SRES scenarios included in the figure are A2 and A1B, and those actually track CO₂ concentrations quite well. The disagreement is primarily with 1970s and 1980s era models; however, as Figure 1 in Hausfather et al. (2019) notes the trend in total forcing in these early models is actually a tad low due to their exclusion of non-CO₂ GHGs.

Quote given to Carbon Brief

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3.2 Future emission scenarios and the carbon cycle

Page 15

MISLEADING

The implausibility of the RCP8.5 scenario was examined by Burgess et al. (2021). The implausibility of RCP8.5 should not be interpreted as very unlikely (e.g. 95th 23 percentile) or a “worst case”, but rather as genuinely implausible owing to the implausibility of the inputs required to reach a forcing of 8.5 W/m². They noted that RCP8.5 has already diverged from observed trends in energy use and the near future trends diverge sharply from those of the International Energy Agency (IEA), which provides market-based projections of energy use for the coming decades. Pielke Jr. et al. (2022) further showed that the historic and projected IEA trends run near the bottom of the

envelopes of both RCP projections and the more recent Shared Socioeconomic Pathway (SSP) scenario trends.

[[HYPERLINK "https://berkeleyearth.org/team/"](https://berkeleyearth.org/team/) \l "zeke-hausfather" \h],
research scientist, Berkeley Earth

The authors do not clearly differentiate between the implausibility of a particular emissions scenario like that used to generate RCP8.5 and the forcing outcome of 8.5 watts per meter squared. The latter can come about not just through emissions, but also carbon cycle feedbacks (Hausfather and Betts (2020)). While it remains quite unlikely in my view that we end up at 8.5W/m² by 2100 even with large carbon cycle feedbacks, it is not impossible under a RCP6.0 emissions pathway. Similarly, 8.5W/m² radiative forcing becomes increasingly plausible post-2100 if emissions continue.

Quote given to Carbon Brief

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3.2 Future emission scenarios and the carbon cycle

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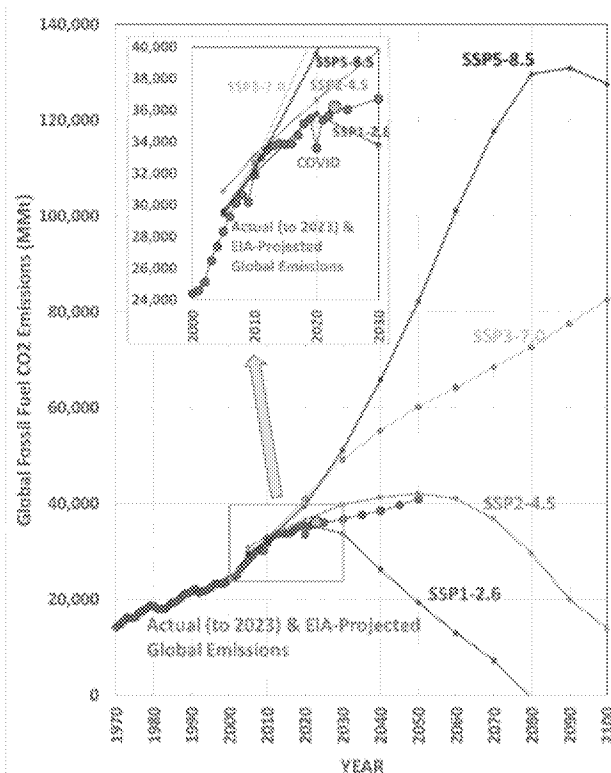


Figure 3.2.2. Observed and projected CO₂ emissions. Source: IPCC (SSP scenarios) and Energy Information Administration (EIA). Green: observed historical emissions and EIA projections. Other lines: SSP1-5. Data source: Friedlingstein *et al.* (2024).

[[HYPERLINK "https://berkeleyearth.org/team/"](https://berkeleyearth.org/team/) \l "zeke-hausfather" \h], research scientist, Berkeley Earth

The authors mistakenly conflate total global observed CO₂ emissions in this text with CO₂ emissions from fossil fuels alone (which are shown in Figure 3.2.2). If total CO₂ emissions are used (including land-use emissions), then CO₂ emissions fall right in the middle of the SSPs; in-line with SSP2-4.5 and SSP4-6.0, below SSP3-7.0 and SSP5-8.5, but above SSP1-1.9, SSP1-2.6 and SSP4-3.4.

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3.2 Future emission scenarios and the carbon cycle

Page 17

MISLEADING

There are about 850Gt of carbon (GtC) in the Earth's atmosphere, almost all of it in the form of CO₂. Each year, biological processes (plant growth and decay) and physical processes (ocean absorption and outgassing) exchange about 200GtC of that carbon with the Earth's surface (roughly 80GtC with the land and 120GtC with the oceans). Before human activities became significant, removals from the atmosphere were roughly in balance with additions. But burning fossil fuels (coal, oil, and gas) removes carbon from the ground and adds it to the annual exchange with the atmosphere.

[[HYPERLINK "https://www.agu.org/user-profile?cstkey=E0190A3A-0069-416C-8B0C-4048C7E69CB8"](https://www.agu.org/user-profile?cstkey=E0190A3A-0069-416C-8B0C-4048C7E69CB8) \h], retired atmospheric physicist

The total mass of carbon in the atmosphere is increasing over time as the carbon dioxide concentration increases. Crisp et al. (2022) quoted a mass of 877 Peta grams of Carbon (Pg C; one Pg C is equivalent to one billion tonnes of carbon (GtC) or 3.67 billion tonnes of CO₂), which was a good estimate in 2020, when the atmospheric CO₂ dry air mole fraction (a measurement of concentration) was around 412ppm. Here, their estimate of “about 850GtC”, was relevant around a decade ago, when the average CO₂ concentration was closer to 400ppm. The quoted gross annual exchange of carbon between land and the atmosphere and between the ocean and the atmosphere are erroneous and their relative values are reversed. Here, they state that land exchanges 80GtC with the atmosphere and the oceans exchange 120GtC with the atmosphere. They do not cite specific sources for these numbers. They do not

come from Crisp et al. (2022). That paper cites gross ocean-atmosphere carbon flux of 90GtC per year and gross land-atmosphere carbon fluxes between 120 and 175GtC per year.

They end this paragraph by noting that the 10.3GtC emitted by fossil fuel use and cement manufacturing is only about 5% of annual emissions by the land biosphere and oceans. This is a half-truth that is clearly intended to be misleading. It fails to recognise that the atmosphere and climate respond to the net emissions and removals of carbon, not the gross fluxes. The land biosphere and ocean both add and remove carbon from the atmosphere, while fossil fuel use and other human activities only add carbon to the atmosphere. When averaged over the globe and over the year, the land biosphere and ocean remove as much carbon as they emit, along with over half of these anthropogenic emissions, yielding net emissions that are negative and only about half as large as the positive anthropogenic emissions. In this context, the net anthropogenic emissions are more than twice as large as the net natural CO₂ fluxes.

Quote given to Carbon Brief

Supporting evidence

[HYPERLINK "<https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2021RG000736>" \h]

3.2 Future emission scenarios and the carbon cycle

Page 18

MISLEADING

The carbon cycle accommodates about 50% of humanity's small annual injection of carbon into the air by naturally sequestering it through plant

growth and oceanic uptake, while the remainder accumulates in the atmosphere (Ciais et al. (2013)). For that reason, the annual increase in atmospheric CO2 concentration averages only about half of that naively expected from human emissions.

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Here, the report authors recognise that existing observations show that natural processes in the land biosphere and ocean consistently remove about 50% of the anthropogenic CO2 emissions. They also note that this ratio has been preserved as the fossil fuel emissions have increased. They attribute the increasing land uptake to the “global greening” phenomena described in chapter 2.1 of this work. However, that chapter fails to recognise that the increasing leaf area index (LAI) does not always indicate increased CO2 uptake. In particular, as noted in Crisp et al. (2022), many regions of the tropics, where the largest increases in LAI have been seen over the past 50 years, have started to transition from net CO2 sinks to net sources of CO2. These changes are due to the impacts of human activities (e.g. deforestation, forest degradation) and climate change (increasing temperature, drought, vapor pressure deficit).

The authors also fail to note that while tropical land is absorbing less CO2, these losses are being compensated mostly by increased carbon uptake by forests at mid and high latitudes. There, climate change is driving longer, warmer growing seasons and permafrost thaw is allowing trees to grow deeper, more massive root systems that sequester more carbon in the soil. The rapid growth, combined with higher temperatures and more frequent droughts at these latitudes, result in larger and more frequent wildfires, which release CO2.

In summary, while the fraction of the anthropogenic CO₂ that has been absorbed by the land biosphere has remained almost constant in response to increasing atmospheric CO₂ abundances, different parts of the world are responding differently to climate change. The focus on “global greening” is a massive oversimplification of the system.

Quote given to Carbon Brief

Supporting evidence

[[HYPERLINK "https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2021RG000736" \h](https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2021RG000736)]

3.2 Future emission scenarios and the carbon cycle

Pages 28-31

MISLEADING

Section 3.2.2: subsections on “CO₂ uptake by land processes” and “CO₂ uptake by ocean processes”

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The last two subsections of this section focus on the carbon cycle models used by the Global Carbon Project. For both land and ocean, they note that this set of models predicts a range of estimates for the decadal uptake of CO₂ by the land and ocean carbon sinks. They use these results to foster doubt in the utility of these methods for predicting future change in the carbon cycle as it responds to climate change. This is interesting because they fail to note that all of the Dynamic Global Vegetation Models (DGVMs) included in this set include CO₂

fertilisation – a mechanism that the authors of this document advocate as a key benefit of increasing atmospheric CO2 amounts.

Quote given to Carbon Brief

3.3 Urbanisation influence on temperature trends

Page 20

MISLEADING

Peterson et al. had failed to find any difference in trends between rural and urban samples, although their definition of rural included local populations up to 10,000 persons while the relative influence of urbanisation begins well below that (Spencer et al. (2025)).

[[HYPERLINK "https://berkeleyearth.org/team/"](https://berkeleyearth.org/team/) \l "zeke-hausfather" \h], research scientist, Berkeley Earth

While Peterson et al. (1999) used a single proxy for urbanity, the follow-up Hausfather et al. (2013) study used four different proxies for urbanity in the conterminous US. It found little residual urban heat island (UHI) bias in the homogenised NOAA data, even when only rural stations are used for breakpoint detection and correction in the homogenisation process to avoid any risk of aliasing in a UHI signal.

Quote given to Carbon Brief

Supporting evidence

[[HYPERLINK "https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2012JD018509"](https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2012JD018509) \h]
